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ROYAL COMMISSION ON VIVISECTION.

SECOND REPORT OF THE COMMISSIONERS.

Presented to both Houses of Parliament by Command of His Majesty.



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1907.

ROYAL COMMISSION.

WHITEHALL, 17TH SEPTEMBER 1906.

The KING has been pleased to issue a Commission under His Majesty's Royal Sign Manual to the following effect :—

EDWARD, R. & I.

EDWARD THE SEVENTH, by the Grace of God, of the United Kingdom of Great Britain and Ireland and of the British Dominions beyond the Seas King, Defender of the Faith, to

Our right trusty and well-beloved Cousin and Councillor WILLIAM COURT, VISCOUNT SELBY ;

Our right trusty and well-beloved Councillor AMELIUS MARK LOCKWOOD, Commander of Our Royal Victorian Order, Honorary Colonel of the 4th Battalion of the Essex Regiment ; and

Our trusty and well-beloved :—

SIR WILLIAM SELBY CHURCH, Baronet, Knight Commander of Our Most Honourable Order of the Bath, Doctor of Medicine ;

SIR WILLIAM JOB COLLINS, Knight, Doctor of Medicine ;

SIR JOHN MCFADYEAN, Knight ;

MACKENZIE DALZELL CHALMERS, Esquire, Companion of Our Most Honourable Order of the Bath, Companion of Our Most Exalted Order of the Star of India, one of the Under-Secretaries of State to Our Principal Secretary of State for the Home Department ;

ABEL JOHN RAM, Esquire, one of Our Counsel learned in the Law ;

WALTER HOLBROOK GASKELL, Esquire, Doctor of Medicine ;

JAMES TOMKINSON, Esquire ; and

GEORGE WILSON, Esquire, Doctor of Medicine ;

GREETING !

Whereas We have deemed it expedient that a Commission should forthwith issue to inquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise ; and also to inquire into the law relating to that practice, and its administration ; and to report whether any, and if so what, changes are desirable ;

Now know ye that We, reposing great trust and confidence in your knowledge and ability, have authorized and appointed, and do by these Presents authorize and appoint you, the said William Court, Viscount Selby (Chairman) ; Amelius Mark Lockwood ; Sir William Selby Church ; Sir William Job Collins ; Sir John McFadyean ; Mackenzie Dalzell Chalmers ; Abel John Ram ; Walter Holbrook Gaskell ; James Tomkinson ; and George Wilson to be Our Commissioners for the purposes of the said inquiry.

And for the better effecting the purposes of this Our Commission We do by these Presents give and grant unto you, or any five or more of you, full power to call before you such persons as you shall judge likely to afford you any information upon the subject of this Our Commission ; and also to call for, have access to and examine all such books, documents, registers and records as may afford you the fullest information on the subject, and to inquire of and concerning the premises by all other lawful ways and means whatsoever.'

And We do by these Presents authorize and empower you, or any five or more of you, to visit and personally inspect such places as you may deem it expedient so to inspect for the more effectual carrying out of the purposes aforesaid.

And We do by these Presents will and ordain that this, Our Commission, shall continue in full force and virtue, and that you, Our said Commissioners, or any five or more of you, may from time to time proceed in the execution thereof, and of every matter and thing therein contained, although the same be not continued from time to time by adjournment.

And We do further ordain that you, or any five or more of you, have liberty to report your proceedings under this Our Commission from time to time, if you shall judge it expedient so to do.

And Our further will and pleasure is that you do, with as little delay as possible, report to Us under your hands and seals, or under the hands and seals of any five or more of you, your opinion upon the matters herein submitted for your consideration.

And for the purpose of aiding you in your inquiries We hereby appoint Our trusty and well-beloved Charles Clive Bigham, Esquire, Companion of Our Most Distinguished Order of Saint Michael and Saint George, Captain in Our Army, to be Secretary to this Our Commission.

Given at Our Court at *St. James's*, the 17th day of September 1906, in the sixth year of Our Reign.

By His Majesty's Command.

H. J. GLADSTONE.

Note.—His Majesty was subsequently pleased to create Mr. Chalmers a Knight Commander of the Most Honourable Order of the Bath.

ROYAL COMMISSION ON VIVISECTION.

SECOND REPORT.

TO THE KING'S MOST EXCELLENT MAJESTY..

MAY IT PLEASE YOUR MAJESTY,

Availing ourselves of Your Majesty's permission to report our proceedings from time to time, we beg to submit to Your Majesty, as an Appendix to this our Second Report, Minutes of the Evidence taken during the months of February and March, 1907, as we think the present publication of these documents is desirable.

ALL WHICH WE HUMBLY SUBMIT FOR YOUR
MAJESTY'S MOST GRACIOUS CONSIDERATION.

SELBY (*Chairman*).

M. LOCKWOOD.

W. S. CHURCH.

W. J. COLLINS.

J. McFADYEAN.

M. CHALMERS.

A. J. RAM.

WALTER H. GASKELL.

JAMES TOMKINSON.

GEORGE WILSON.

CLIVE BIGHAM (*Secretary*).

15 April 1907.

ROYAL COMMISSION ON VIVISECTION.

APPENDIX

TO

SECOND REPORT OF THE COMMISSIONERS.

MINUTES OF EVIDENCE,

February to March, 1907.

Presented to both Houses of Parliament by Command of His Majesty.



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1907.

THE COMMISSIONER OF THE GENERAL LAND OFFICE

REPORT

SECOND REPORT

THE COMMISSIONER OF THE GENERAL LAND OFFICE

REPORT

THE COMMISSIONER OF THE GENERAL LAND OFFICE

REPORT



REPORT

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ROYAL COMMISSION ON VIVISECTION.

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ROYAL COMMISSION ON VIVISECTION.

LIST OF WITNESSES.

In the order in which they appeared before the Royal Commission.

Date.	Name of Witness.	Profession, Occupation or Residence.	Representing.	Number of first Question.	Page.
1907. 10th Day - Feb. 20th.	Mr. W. H. POWER, C.B., F.R.S.	Principal Medical Officer to Local Government Board.	Local Government Board.	4,281	1
11th Day - Feb. 26th.	Mr. A. R. CUSHNY, M.A., M.D.	Professor of Pharmacology and Materia Medica in University College, Lon- don.	Committee of Medi- cal and Scientific Societies.	4,640A	17
12th Day - Feb. 27th.	<i>Mr. A. R. Cushny</i> (re- called).	- - - - -	- - - - -	5,154	35
	Miss A. KENEALY, L.R.C.P. (Dublin).	Authoress and Journalist -	Parliamentary As- sociation for the Abolition of Vivi- section.	5,275	39
13th Day - Mar. 5th.	The Right Hon. The Lord RAYLEIGH, O.M.	President of the Royal Society.	The Royal Society -	5,530	53
	Sir R. DOUGLAS POWELL, Bart., K.C.V.O., M.D.	President of the Royal Col- lege of Physicians.	The Royal College of Physicians.	5,580	55
	Mr. FREDERICK TAYLOR, M.D., F.R.C.P., M.R.C.S.	Senior Physician to Guy's Hospital.	- - - - -	—	55
14th Day - Mar. 6th.	Mr. J. W. GRAHAM, M.A.	Principal of Dalton Hall, Victoria University, Man- chester.	Parliamentary As- sociation for the Abolition of Vivi- section.	5,835	65
15th Day - Mar. 12th.	<i>Miss A. Kenealy</i> (re- called).	- - - - -	- - - - -	6,164	78

MINUTES OF EVIDENCE

TAKEN BEFORE THE

ROYAL COMMISSION ON VIVISECTION.

TENTH DAY.

Wednesday, 20th February, 1907.

PRESENT :

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. J. TOMKINSON, M.P.
Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G., *Secretary*.

Mr. W. H. POWER, C.B., F.R.S., called in; and Examined.

4281. (*Chairman*.) You are Medical Officer of the Local Government Board?—I am.

4282. And are you licensed under the Act?—Not personally.

4283. Has the Local Government Board laboratories of its own?—No. At one time the Board used to hire laboratories for their pathological work, but of late years they have made an agreement with each of the investigators which covers not only his personal remuneration, but his working laboratory expenses, and he has to make his own arrangements with some public laboratory in which he carries on his work.

4284. What is the name of the gentleman you are speaking of?—I am speaking of a series of gentlemen. We employ several. One, Dr. Sidney Martin, works at University College Laboratory; he is Professor of Pathology there; and two of them work at the laboratory at St. Bartholomew's Hospital; and one was working at the Lister Institute.

4285. I think you state in the paper you have sent us that is done by piece-work?—Yes, that is piece-work.

4286. And you, I gather, do not operate yourself?—No.

4287. Do you superintend these operations, or do you merely say "I wish such and such experiments to be made"?—I do not go further than considering the work to be done, and contriving it to a certain extent, and arranging it with the actual investigator chosen for it, estimating its cost and the number of animals and the sort of animals that will be used. Then he gets a letter of engagement from the Local Government Board to undertake this work for a certain sum, including expenses. It is his business to get a licence for himself and for the laboratory, if necessary, where he works, and to get the necessary certificates. That is done between him and the Home Office.

4288. And do you feel it any part of your duty to superintend the way in which he performs his work, to see, for example, whether he conforms to the Act?—No, I do not personally do that, but I look very strictly after the result, as to what the several experiments were, and I get a full report from him of all the work he has undertaken. This appears in the Medical Officer's annual report which is laid before Parliament; and usually I comment upon the work of each operator.

4289. I have not quite come to that yet. The Act requires, as you know, in certain cases certificates to be obtained?—Yes.

4290. And in all painful cases, anæsthetics are to be used; of course, you are aware of that?—Yes, I am quite aware of that. I send this reminder to each worker on each occasion of his engagement; it is a reminder of what he is under obligation to do. (*Handing in a paper.*)

4291. It is, I see, an extract from a letter which is always sent to scientific workers for the Board: "I am to remind you that no experiments on living animals are to be conducted at the cost of the State without the employment of anæsthetics in the case of painful operations, nor without a report to me from time to time explaining the object of any such experiments made by you, and showing their necessity for the purpose of discovery." That is part of the instructions?—That is part of the instructions on each occasion of engagement.

4292. And beyond that I understand, as you say, that you exercise no superintendence over the methods of performing the operation or of compliance with the Act?—Most of the operations which are done for us are mere pricks or feedings, but in the particular instance of a test of blood pressure or anything of that kind where anæsthetics would be required, or a dog or cat used, I expect to be informed of it by the investigator before he undertakes it.

4293. By "informed" do you mean informed that it will be necessary to perform such and such a painful operation?—That he is going to do it. I should like to know if he is going to undertake something quite out of the ordinary course altogether with us. Most of it is merely a syringe prick of a guinea-pig or rabbit. We do very little beyond that.

4294. But any painful cutting operation you would expect to be informed about?—I would expect to be informed about a painful cutting operation.

4295. And being informed, would you exercise any control over it?—I should want to know the result.

4296. After it is over?—After it is over. I will hand in, if you will allow me, a statement of the kind to which I refer. (*Handing in the same.*)

4297. I want now to ask you a few general questions. How long have you been Medical Officer?—Since January, 1900.

Mr. W. H.
Power,
C.B., F.R.S.

20 Feb. 1907.

Mr. W. H.
Power,
C.B., F.R.S.

20 Feb. 1907.

4298. And you are a Fellow of the Royal Society and a Fellow of the Royal College of Surgeons?—I am.

4299. Would you tell us first what are the functions of the Local Government Board as regards experiments? For what purposes have they to make experiments, and with what object?—The principles which have governed always the pathological investigations, formerly of the Privy Council and later of the Local Government Board are best set out in Sir John Simon's own words. In an explanatory minute, dated 26th February, 1875, he says: "In modern endeavours to increase the power of preventing different diseases of men and domestic animals, usually a first aim is to obtain exact scientific knowledge of the causes and the mode of attack of any disease which is in question; and in this sort of study it frequently happens that more or less experiment has to be made as to the results which the administration of a particular influence will produce on an animal. In further aid of preventive medicine, and often in aid of therapeutics, experiments on animals are also from time to time wanted to test, as against known causes or processes of disease, the value of alleged prophylactics, antidotes, and remedies. Studies of this experimental test are made sometimes more immediately in the interests of man, as for instance, in the case of Asiatic cholera, and sometimes more immediately in that of the domestic animals, as in the cases of sheep-pox and cattle plague, but perhaps oftenest in the common interest of both; for, in regard of certain large quantities of disease, it may be assumed that any better knowledge which is got will probably sooner or later yield equal advantage to human and to veterinary medicine. If in such experiments it were ever necessary to perform on any animal any severely painful operation, chloroform or other anæsthetics ought, of course, to be used; in fact, experiments of the class referred to have generally consisted only in inoculating animals (by mere puncture) with some specific material or in giving to them in their food the material of which the effect has had to be watched, and perhaps afterwards taking from them blood for examination, operations which in themselves are little or nothing more than the vaccination or bleeding of the human subject. No doubt, however, but that pain or uneasiness will afterwards arise in cases where disease is the result of the experiment; but in experiments which are not of curative intention, the experiment would often require that the animal should be killed for examination as soon as disease had become manifest, and, of course, no animal would needlessly be let live in a state of suffering. That experimental studies of disease are of the utmost importance to the progress of medicine, that indeed such progress must at present in large proportion depend on them, is, in my opinion, quite certain. For myself I may say that, obliged as medical adviser of the Government to make myself as proficient as I can in all that relates to the preventing and resisting of disease, I have felt it indispensable that I should have recourse, *inter alia*, to such studies; and in some of the scientific investigations which are made under authority of the Privy Council in aid of pathology and medicine, and which I have the honour of superintending for their Lordships, such studies have necessarily formed a part. I can confidently say that in any experimental use which has thus been made of animal life in the medical interests of mankind, conscientious regard has always been had to the limit within which such use can alone, in my opinion, be justified. In view of the universal practice of mankind in other relations to the lower animals, and under sanction as I believe of sound reason, it has been assumed that, for the sufficient advantage of man, suffering may be inflicted on brute animals; but the limitations have been well remembered, that no such suffering should be inflicted except in endeavours for considerable human advantage, nor except with all proper care, to make the suffering as little as can be. At the end of 1873, on the occasion of a provisional statement which I then made (and which has since been embodied in a report laid before Parliament) on the scientific investigations which were in progress under the Privy Council, Mr. Forster asked me to explain to him how I stood in regard of the performance of painful operations on animals; and on the explanation which I then gave to him in accordance with that which I have here written, he wrote remarks to be presently quoted. I communicated Mr. Forster's minute as an instruction to the gentlemen who act with me in pathological investigations."

Mr. Forster, as Sir John Simon states, made

a special note on the use of animals in some of the investigations. His observations were the following:—"Upon being informed that there were experiments on living animals connected with the Auxiliary Scientific Investigations, I had to consider how far I could make myself in any way responsible for these experiments. After careful consideration, I came to the conclusion that however much I might dislike vivisection I should not be justified in preventing it if practised, not only for the purpose, but with the reasonable ground for expectation that it may result in discoveries which will diminish disease in men or other animals, provided that every precaution against pain be taken of which the experiment admits. As such discoveries are the object of these investigations, and, indeed of the vote, I felt I could not require these experiments, when really necessary, to be discontinued, but I thought it right to require that they should be conducted with every endeavour to avoid pain, and especially with the use of chloroform or other anæsthetics. I found that these experiments were thus conducted, as indeed I expected would be the case, knowing as I do the desire of Mr. Simon to avoid giving pain to any animal, and it is therefore merely as a justification for myself that I desire to have on record the opinion upon which, had I remained in office, I should myself have acted—viz., that no experiments on living animals should be conducted at the cost of the State without the employment of some anæsthetic in case of painful operation, and without a report from time to time by gentlemen conducting the experiments, explaining their object, and showing their necessity for the purpose of discovery." That is dated the 17th of February, 1874, and signed "W. E. F."

At a later date, 1881, in the State Medicine Section of the International Congress, Sir John thus explained the sources of experience relied on by the Department of which he had been chief. "The experiments which give us our teaching are of two sorts—on the one hand we have the carefully pre-arranged and comparatively few experiments which are done by us in pathological laboratories, and for the most part on other animals than man; on the other hand, we have the experiments which *accident* does for us, and above all, the incalculably large amount of crude experiments which is *popularly done by man on man* under our present ordinary conditions of social life, and which gives us its results for our interpretation." Since Sir John Simon's day, work of an administrative character has tended more and more to occupy the Board's Medical Staff. It is field observation of experiments of the latter of the two classes referred to by Sir John Simon which continues to employ no inconsiderable portion of the time of our permanent Inspectors. Their business in this connexion is by intelligent observation and record of the facts which come before them to trace effects to their causes; work which differs rather in degree than in kind (differs only perhaps as regards the level at and scale on which it is carried on) from that performed day by day by the medical profession generally. Investigation of the behaviour of epidemic disease in town and country is the class of work particularly in question. As to other investigatory work under the Department, namely, carefully pre-arranged experiments that are done in pathological laboratories, these are not now any of them carried out by the Board's Inspectors. They are entrusted to special investigators of highly trained intelligence and approved competence who are engaged from year to year in "piece work" of the class in question.

4300. Why do you say they are not now carried out by the Board's Inspectors. Were they formerly so carried out?—Formerly in Sir John Simon's day they were. Sir John Burdon Sanderson, for instance, acted as medical inspector and also as pathological investigator. Very largely such experiments deal with particular causes of disease in their relation to the animal bodies on which they are made to operate, with special reference in almost every case to the means by which their effect is manifested, and very often the effect of environment on them. The studies now undertaken for the Board in this connection are of two classes—investigation of problems the prompt solution or provisional solution of which promises to be of immediate administrative advantage to the Board in the exercise of their public health functions; and studies which from their nature cannot promise rapid results, but require systematic and continuous labour extending over considerable periods of time. A

scheme of work in the above sense for 1901-2 (the first year of my more direct responsibility in these matters) serves to illustrate the considerations which the Board are accustomed to hold in view and the means taken to give effect to them.

4301. You are prepared, I understand, to give the Commission an example of the scheme of work to which you refer?—If you please. It is as follows:—

Year 1901-2.—Research for Administrative Purposes.
—A. To ascertain whether carbonic oxide (the “after damp” of mines) may be utilised for the destruction in ships’ holds of rats infesting vessels’ cargoes; especially what are the obstacles, if any, to the application of such method of killing rats to vessels of large size having their cargoes *in situ*. *Result.*—“Report by Dr. Haldane on the application of carbonic oxide to the destruction of rats on plague-infested vessels.” (Animal experiment, nil.) B. Identification and differentiation *bacterioscopically* (apart from the animal body) of particular microbes, in view of need for a standard test in this matter having a sufficient basis of authority. (a) Investigation of anaërobic microbes which occur in sewage, in water, and in food stuffs, with a view to ready differentiation of those pathogenic (to man) and those innocent. *Result.*—“Report by Dr. Klein on the differentiation of the several anaërobic microbes commonly present in the intestinal contents of man and other animals.” (Animal experiment on guinea-pigs.) (b) Study of excremental aerobic *B. coli* derived from various sources, with a view to determining strains recently in association with *B. typhosus* and for the purpose of obtaining an index as to the presence or absence in any material of the microbic cause of enteric fever which is so difficult of detection. *Result.*—“Report by Dr. Klein on agglutination by blood of emulsions of microbes, with special reference to specificity.” (Animal experiment on rabbits and guinea-pigs.) (c) Search among bacilli liable to inhabit the throat of the human subject for means of readily distinguishing bacteria which are derivatively and potentially diphtheria bacilli from those other bacteria which, though non-pathogenic, have been confounded with the microbic cause of diphtheria. *Result.*—“Report by Dr. Mervyn Gordon on bacillus diphtheriae and micro-organisms liable to be confounded therewith.” (Animal experiment on guinea-pigs.)

Research for Prophylactic Purposes.—Plague, as well as enteric fever, threatened to call for preventive inoculation of individuals. For plague, the preventive then available was Haffkine’s artificial culture of *B. pestis* plus the products of these bacilli, sterilised. For enteric fever there was Wright’s cultivation of the “typhoid bacillus.” Neither had proved altogether satisfactory. Against Haffkine’s cultures it had been urged that they comprised in uncertain amounts two principles: The one bound up with the bodies of the bacilli, the other an element of their chemical products, and that the virtue of the one and the other principle was not separately known. Against the enteric fever prophylactic was the fact that it had not uncommonly been found less potent for its purpose than is desirable, and that a more trustworthy preventive of the disease was being demanded. Hence (a) Investigation of the nature and their effects on the animal body of the active principles contained in Haffkine’s plague prophylactic; and (b) experiment as to the inhibitory effect, within the animal body, on the typhoid fever bacillus, of antecedent, simultaneous and subsequent introduction of the chemical products of those soil bacteria which Dr. Sidney Martin had found in the aggregate absolutely germicidal in the laboratory to the particulate cause of enteric fever. *Results.*—(a) “Report by Dr. Klein on the nature of Haffkine’s Plague Prophylactic” (Animal experiment on rats, guinea-pigs, and rabbits). (b) Statement by Dr. Martin that whereas alcoholic extract of the mixed chemical products in culture of soil bacteria were found by him inhibitive of the growth of the typhoid bacillus, a prolonged attempt by him to separate out the germicidal elements of such products had failed. He had been unable to obtain a relatively pure product having ability to inhibit the growth of *B. typhosus*. (Animal experiment nil.)

Research for Investigatory Purposes.—Work under this heading had to do with investigations of less immediate promise of useful application than the foregoing, but which nevertheless might yield results of high interest, and at the same time throw light on problems maturing for the Board in the future. The researches in question comprised:—(a) Resumption

by Dr. Sidney Martin of his study of the chemical pathology of infectious disease, with preference in the first instance for diarrhoea of one and another class in reference to its microbic cause. *Result.*—“Report by Dr. Martin on the Chemical Products of Diarrhoea-producing Bacteria.” (Animal experiment on guinea-pigs and rabbits.) (b) Continuation by Dr. Houston of work on which he had been engaged previously. (1) Investigation of the fate of sewage microbes in soil. (a) The soil experiments to be, if practicable, with a sandy virgin soil, free comparatively from spores of *B. enteritidis sporogenes*. (b) The soil to be tested before, during, and after inoculation with sewage, as to (i.) total bacteria; (ii.) spores of aerobic bacteria; (iii.) spores of *B. enteritidis*; (iv.) presence of streptococcus; (v.) presence of *B. coli* and of coli-like microbes; (vi.) relative amounts of gas-forming and indol-forming bacteria. (2) Further investigation of Chichester well waters. Chemically as to (i.) free and albuminoid ammonia; (ii.) chlorin; (iii.) oxygen absorbed from permanganate. Bacterioscopically as to (i.) *B. coli* and coli-like microbes; (ii.) Streptococcus; (iii.) *B. enteritidis sporogenes*. (Special attention to be given to coli-like microbes, and *B. typhosus* to be diligently sought for.) *Result.*—“Report by Dr. Houston on Inoculation of soil with Sewage”; and “Report by Dr. Houston on Chemical and Bacteriological Examination of Chichester Well Water.” (Animal experiment nil.)

In addition, in 1901-02 the Board, in the exercise of their function of safeguarding the country against exotic disease, were called on to make use to an exceptional extent of animal experiment. By international agreement the Foreign Office is under obligation to notify at once to other Governments all plague (as also cholera) making its appearance in this country, whether directly imported or arising as it were indigenously. And accordingly the Board in turn are under obligation to keep the Foreign Office informed of all plague occurrences in England and Wales. When therefore plague began to threaten us at the beginning of the century the Board sought, by circular to local authorities, immediate information of all illness suspected to be plague, and at the same time made arrangements for reception by their medical officer, with a view to submission to adequate test, of clinical and pathological material from any such cases arriving in our ports or developing within the districts of inland authorities. Immediate information and prompt test of all suspected plague was requisite, not only for the discharge of England’s international obligations, but also in the interests of public health and of commerce; it being of importance that plague-infected vessels should be at once fully dealt with, and vessels found not infected as quickly released. The tests of suspected plague material employed under the Board involve in almost every instance intraperitoneal injection of guinea-pigs, and in this way during the year a dozen or two of these animals were made the subject of experiment by Dr. Klein. Suspected plague occurrences in England and Wales in 1901 numbered 46, all but 13 of them being instances of ship-borne disease.

4302. Is there now any medical staff of the Board except yourself?—There is a medical staff. Besides myself there are two assistant medical officers and 13 medical inspectors.

4303. In different parts of the country?—No, they nearly all of them work from town.

4304. And would you tell us, keeping so far as you can to those duties which involve experiments upon animals—take the case, for instance, of a disease that one hears a great deal about just now, spotted fever, would the Local Government Board take up that question on being told by their medical officers that this disease is showing itself or prevailing?—The Board would probably be prepared to have examined by competent investigators material from the disease, the cerebro-spinal fluid, or the blood, as the case may be, with a view of identifying the disease on its first appearance in a locality. That we have done with other diseases, for instance diphtheria. That is where the pathological experiment would come in in connection with cerebro-spinal fever.

But we investigate locally by our medical inspectors the conditions under which disease exceptionally prevails. We should send an inspector down, as we have done already in the case of this malady. Cerebro-spinal fever is no new thing in this country. In the last 15 or 20 years it has been cropping up here and there, and on several occasions we have sent medical inspectors to investi-

Mr. W. H.
Power,
C.B., F.R.S.
20 Feb. 1907.

Mr. W. H.
Power,
C.B., FR.S.

gate the circumstances under which it has been occurring. Only a few months ago we found some at a place called Irthlingborough, in Northamptonshire.

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4305. I did not wish to go too much into the history of that particular disease, but when that or any other particular disease appears which seems to require particular investigation, which is or is thought to be new, for which remedies have not been explored sufficiently, what do you do that involves experiments?—Then we should send a medical inspector in the first place to investigate the circumstances, and arrange for our having material from some of the cases if it required to be identified; either some of the living tissues with certain fluids, or it might be swabs from the throat in living cases, or post-mortem material from cases that died; and on those we should employ one of our special investigators, not a medical inspector.

4306. I come now to the experimenter, would he experiment with that on guinea-pigs?—Guinea-pigs or rabbits mostly, and also by seeking to isolate the micro-organism that was believed to be at fault in artificial culture.

4307. Would that be with the object of discovering the cause of the disease or discovering a remedy for it, or both?—It would be with a view to identifying the actual disease that was current, and also to experiment probably with material with a view to getting some preventive serum or what not with a view of preventing the disease in other cases. The experiments and observations would go on concurrently. In the first place we should be concerned to identify the disease, supposing it was said to be cerebro-spinal fever, in order to be quite sure that this was the disease prevailing in the given district. Afterwards, if thought desirable, we should proceed to seek if we could a serum or some inoculation fluid that might be of use in arresting extension of the disease.

4308. But your experiment on an animal would be, in the first place, giving it or attempting to give it the disease?—Yes, it would be attempting to give it the disease either with material directly from a human case, or with the culture of such material on an artificial medium.

4309. And then you would try to discover remedies also for it, you say?—Rather a preventive than remedies. We do not consider that we are so much concerned with curative medicine as with preventive medicine. I should like to get some prophylactic material, of course, for all these diseases.

4310. Supposing that you have satisfied yourself that some remedy is a prophylactic (if that is not a contradiction in terms), that you have discovered a prophylactic, do you then recommend it in the name of the Local Government Board to all the local public bodies in the kingdom?—Probably we should write a report on it, and as in the case of the diphtheria preventive we should announce that we had a certain stock of it which might be available in an emergency for local authorities. We did the same with the plague preventive; we announced that we should, at any rate for the first beginnings of the disease, be able to supply a certain amount of the protective material for the injection of "contacts" with the case. We could not supply an unlimited amount, of course; but while other people were seeking this material in other directions the Government would at any rate be supplying some of the material for checking the disease at its outset.

4311. Have you made many recommendations of prophylactics or remedies in the case of disease?—Yes, we have recommended the diphtheria antitoxin, and provided the diphtheria antitoxin. We have made some cholera preventives, Haffkine's method; and again we have done the same in regard to plague. We have prepared material.

4312. Taking those cases, would you recommend them?—When we have proved them.

4313. To your satisfaction and to those of your assistants, I suppose?—Yes, quite. It is essential that we should not recommend or issue anything unless we have satisfied ourselves as to its value and efficacy, and as to its involving no particular danger.

4314. In order to do that are experiments on animals necessary?—Almost always.

4315. In all these cases that you have spoken of?—In all these cases I have spoken of it has been essen-

tial to use the body of the lower animal in order to gain our knowledge.

4316. In the way you have described, by inoculation?—Yes, in the way I have described.

4317. Is it part of your duty to watch, or have you the opportunity of watching, the effect of these remedies after you have recommended them to see how far they are successful or unsuccessful?—Certainly, we get reports from the local officers in the districts in which they have been employed as to their efficacy in controlling the disease.

4318. Take those you mentioned; one you mentioned was Haffkine's fluid. You had satisfied yourselves, I presume, when you sent that recommendation out that that was a valuable prophylactic?—We tested it very carefully on the rat before we issued it.

4319. And with many experiments?—A good many experiments. We did some by ourselves and some in conjunction with Haffkine himself, who happened to be over from Bombay at the time.

4320. And all of them on the same animal?—They were all on the rat except that some of the workers offered themselves for the test of experiment, wishing to get such protection as might be had by it, but those were the only human subjects in this country that to my knowledge really were operated on before this preventive was ready for issue.

4321. Do the same answers apply to the prophylactics against diphtheria?—Yes, the same was done with diphtheria.

4322. Were you satisfied with the success of that as a prophylactic?—Of the diphtheria prophylactic we, perhaps, have not so much evidence at present as to its value, at any rate not commensurate with its value as a curative agent. It is of very great value in that sense, but it has been used as a preventive not so much as one would have desired, partly because it has not been made generally available. There is a question as to paying for it by the local authorities.

4323. You are aware, of course, that there is a great deal of controversy between those who approve of experiments on animals and think them valuable for scientific purposes, and those who do not think them valuable, both as to diphtheria antitoxin and Haffkine's cholera antitoxin?—Yes.

4324. Your view is that you thought them successful and recommended them as worthy of trial, and that they have turned out to be successful?—So far as we have had an opportunity of judging of their use as a preventive they have been successful. We have not had much experience of Haffkine's cholera prophylactic in this country, because we have not in recent years had a great deal of cholera, but we made some for anticipated use in inoculation of cholera "contacts" at the ports in this country where ships are liable to arrive with cholera on board.

4325. Do the local boards to whom you send these reports generally or universally act upon them? Could you give us any idea to what extent they have adopted the views of your report on diphtheria and cholera, for example?—They generally adopt them. When I have been in a position to make a certain amount of this, that or the other material, to put at the disposal of port or inland authorities, many of them have welcomed it; certain of our Colonies, too, have pressed for it. But employment of prophylactic on large scale costs money, and it is not issued to a very large extent, but at any rate we have provided enough to deal with the first beginnings of exotic disease whether imported or arising in inland districts.

4326. What was the third disease with regard to which you say you have done the same thing?—Plague was the third.

4327. Do the same answers apply to that?—Yes, the same applies to that.

4328. Both as to the methods pursued and as to the success of them?—Yes.

4329. As to the success I would like to ask you precisely, do you consider that the remedy in that case has been valuable?—As regards the plague prophylactic we have, of course, had very little experience of use of it in this country, but we do not consider that it is perhaps the best thing that can be got; and we have been experimenting lately in getting a plague prophylactic more satisfactory perhaps than Haffkine's fluid, and, so far, with very encouraging results. We

have been taking the actual dead tissues of the plague organs of an experimental animal and drying them under sulphuric acid and then powdering them up and making an emulsion and using it for the injection of animals. We find that a very small quantity of it is highly protective against subsequent inoculation of virulent plague, not only in the rat and partially in the guinea-pig, but also in the monkey, in this instance the nearest approach to man that we were able to test it on.

4330. And has this new remedy been recommended by you?—We are just now reporting upon it, and have issued a preliminary report on its manufacture. We are now about to issue a detailed report upon the subject, and I am going to take the Board's views as to whether we shall manufacture any considerable amount of it.

4331. Do you consider that this discovery of this new remedy that you speak of proves the previous remedy to have been a failure?—No, it is a stronger, I should say, and more manageable prophylactic than the other if it turns out as we expect, and it can be used in a very much smaller amount.

4332. But although this better remedy has been discovered you still think that the prophylactic you had before is valuable?—Yes, and we should continue to issue it. We have it in stock to issue at the present moment.

4333. Has there been recently any great increase in the number of experiments upon animals that the Board have collected?—No, I do not think that from year to year there has been a very large difference. Our investigators, of course, make a return yearly to the Home Office as to the different animals they use and the number of each. I have made a note of one year, 1901; we appear to have used in that year 294 animals out of between 6,000 and 7,000, I think, that were used in the country generally.

4334. (*Colonel Lockwood.*) What is that figure got from?—That is got from the Home Office Return.

4335. (*Chairman.*) Was that 294 an exceptionally small number?—No, I should say that would be an ordinary year. It is the year 1901, and it includes also some animals that these investigators may have used for experiments that were done in investigations that were no concern of the Board's, but done on their own account or for some other purpose.

4336. Then you would not say that it was an exceptional year?—Not as regards the number of animals. I think it is an average year as regards that. I have not checked it very closely, but, of course, we could get the figures for each year.

4337. That is sufficient if you say it is an average year as regards animals. Then as you have stated you have to make experiments for the benefit of the Foreign Office with a view to the Foreign Office supplying information?—Yes, Great Britain is under obligation under the Convention of Venice, which will be superseded shortly by that of Paris, to give immediate notice to foreign Governments of the occurrence of certain diseases in this country, and it is the business of the Local Government Board to keep the Foreign Office informed of such occurrences in England and Wales. Therefore when disease of the class in question is reported to us it is essential that we should as early as possible identify it and report to the Foreign Office accordingly.

4338. Then a certain proportion of these experiments are experiments made for that purpose?—Yes, a certain proportion of them are made for that purpose; and if we had a great run of cholera or of plague on our shores in any given year, of course the number of animals that we used for experiment would be very considerably increased in order to apply the test which would enable us to inform the Foreign Office that they might discharge their obligations to other countries.

4339. You spoke of Tables, and you said that you collected the information from reports that came in to the Local Government Board?—Yes, I issue each year for presentation to Parliament a report which contains among other matters the reports of each of the several investigators and a commentary by myself at the beginning of the volume on each one of the investigations, which are made public; the details are all given by the investigator and there is a commentary by the Medical Officer. The information bearing upon this subject will be found under the head of Auxiliary Scientific Investigations beginning at page xxxiii. of the last Report of the Medical Officer

of the Local Government Board for 1904-5, and under the head of Appendix B at page 295 of the same report; and under similar headings in the previous reports.

4340. You publish every year reports as to experiments?—Every year reports of the work of each of the several investigators in detail as you see them there; and besides that the Medical Officer comments, as you see, on the general outcome of the work.

4341. Do those Tables show on what animals the experiments are made?—Incidentally they speak of the animals.

4342. You can gather that from the text?—Yes, from each report you can see what the animal in question was; it is stated whether it was a rat, a guinea-pig or a rabbit.

4343. What are the animals that are chiefly used?—Almost wholly mice, rats, guinea-pigs and rabbits, I think.

4344. (*Colonel Lockwood.*) And monkeys you said?—Very rarely a monkey. Occasionally there has been a dog or a cat, as in the case I have handed in.

4345. (*Chairman.*) Will it appear whether a dog, a cat or a monkey has been used?—Certainly.

4346. You might let us have a copy of the last report issued?—I will send it; in fact, if you will allow me I will send one for each year since I have been Medical Officer up to the present time.

4347. And that will show also what the nature of the experiment was?—Yes, it is detailed in almost every instance—each step of the experiment with the result to the animal and the final outcome of the observations. I am speaking of the reporter's account of his investigations now, not of the Medical Officer's. Each reporter sends in detail almost every experiment he has made, stating the animal that was used and the effect on the animal.

4348. Are these the same reports which are published by the Home Office?—The Home Office gets the same facts numerically as to the animals which the investigators use.

4349. From the same sources?—From the same workers.

4350. Then all that information already appears in the Home Office reports?—It appears in their annual returns, with the name of the workers, the number of animals used, and the sort of experiments to which they were subjected.

4351. You said that most of these experiments were made simply by inoculating animals to give them the particular disease?—By inoculating or by feeding, but the vast majority, I should say, are subcutaneous inoculations.

4352. When you say the vast majority, would you say 90 per cent?—I should think 80 or 90 per cent. would be subcutaneous inoculations. Occasionally there is intravenous injection, and sometimes, of course, there is a mere cutaneous scratching, but I should think the very minor ones are above 90 per cent.—entirely minor scratchings or pricking or what not. Inclusive of feeding it would be a great deal above 90 per cent.; I should think it would be 98 per cent. inclusive of feeding.

4353. (*Colonel Lockwood.*) You believe that all the experiments on living animals carried out by your piece-work friends—I use your own expression—the gentlemen who work piece-work, are painless experiments, and are carried out strictly according to the regulations laid down for their guidance?—Certainly, if you are speaking of the experiments carried out by the investigators directly employed by the Board for the Board's purposes.

4354. How do you know that?—In the first instance I know that they as experiments are mere pricks, and cannot be painful, and I have, as I was saying, a strict account of everything that is done; and in going over the report of an experiment, I think, I should at once find out or at least get an inkling if anything had been done which should not have been done in the way of causing pain such as the Board desires should be avoided.

4355. Then you go entirely by reports?—I do not personally supervise.

4356. You never have personally supervised?—No. I consider, of course, that the Home Office will be doing that. These men are working under the Home Office

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just like any other investigators. They are liable to visits by the Home Office Inspector; they are visited in exactly the same way; he observes what they are doing, and asks them the why and wherefore of whatever he may see to be going on.

4357. Do you say as your own personal opinion that experiments on living animals are absolutely necessary for Local Government Board purposes?—Yes, we could not do without them.

4358. Why do you say that?—We get a number of questions sent up to us, as I was informing the Chairman just now, and we have to do these experiments as it were under International obligations; and in a great many other cases we could not say yes or no as to the nature of the disease unless we did submit it to test in the animal body. I may say that one object of the Board has been the identification of disease, if possible, without resort to animal inoculation. Accordingly we have done a great deal of work in the direction of observing, in a variety of culture media, and divergencies in mode of growth and the like under varying conditions of suspected micro-organisms, in the hope, I will not say exactly the expectation, for I am afraid it has not been very fully realised, of being able to readily identify disease and differentiate one disease from another without actual resort to inoculation of the animal body.

4359. Why have you delegated your powers to piece-work gentlemen, to these distinguished gentlemen, instead of the Local Government Board carrying them out themselves?—In the first place our medical inspectors would not have time for it, and we have not a scientific investigatory staff apart from our medical inspectors who are permanent salaried officers. Under the piece-work system the Board always desire to employ for work of this kind men specially competent to carry it out; they are men with special training in that direction.

4360. You do not think that your own people are of equal calibre to those gentlemen whom you employ?—On an average, not in this particular; that is, they have not since appointment under the Board continued to give day by day attention to work of this sort.

4361. With regard to the antitoxin remedy for diphtheria, do you consider that has been of undoubted curative value?—Unquestionably.

4362. Have you any statistics which you could put in which go to prove that?—We have not collected the statistics, but the Metropolitan Asylums Board have done so, and they have had a large body of evidence which seems to prove incontestably that if the diphtheria antitoxin is used in the early days of diphtheria it makes an enormous difference in the case mortality of the disease. It reduces what was from 25 to 30 per cent. down to from 5 to 10 per cent.

4363. You can send me some statistics or refer me to some statistics which will carry out what you say?—Yes, the Metropolitan Asylums Board have issued and published very voluminous statistics on the subject.

4364. Do you consider that the success of a remedy on the body of a lower animal ensures its success on the human body?—Not altogether; and it is for that reason that one has some hesitation in applying directly what one learns on the guinea-pig to the human being, and therefore, as I was saying, in the matter of this new plague prophylactic we decided, having obtained it, to try in the first instance on a monkey.

4365. Do you say that the only value of a test as a curative or prophylactic is an experiment on a living animal?—If it satisfactorily protects a lower animal we are at liberty to infer that an adequate and proper dose would protect also the human being, and generally that is found to be the case.

4366. Take Haffkine's experiments at Bombay, for instance. Do you claim a success for those experiments?—I do not know whether Haffkine tested his material in Bombay in the same way in which we tested ours in this country; but certainly its effect in protecting the rat would lead us to infer considerable efficacy in the case of a human being, apportioning the dose to the larger body; though I believe, as a matter of fact, they do not in India inject in absolute amount so much into the human body as we did into the rat.

4367. And you think that because it acted on the rat with success it therefore must act with success on the human body?—The inference was that probably it would do so; and in India it was tried on the human body, and was found very satisfactory. At any rate,

it was the best thing that was going. We are hoping, as I was saying just now, that we may have got something which perhaps is even more satisfactory.

4368. You were saying that when you discovered this valuable remedy either as a curative or a prophylactic, you let the local authorities know that you had discovered something which would be of use in combating any particular disease, as, for instance, diphtheria or cholera; you send it to them, and you say, "We have got a remedy that will assist in this respect"?—It is not quite in that way. The probability is that a prophylactic or a remedy has been advocated from many sources before we actually test it; it is pressed on our notice, and we are urged to put it at the disposal of local authorities. The Board then consider that they are under obligation to test its value before they go as far as that, and it is only after we have tested the value of something that is actually on the market that we are able sometimes to say this does seem to be of use, and we put some of it at the disposal of local authorities.

4369. And when, as in the case of Haffkine's remedy, you think you have got something better than Haffkine's, what do you do; do you write out and say, "We think we have got something better than Haffkine's, so we withdraw our former circular"? The Board have not yet dealt with that matter, but I fancy that the Board would probably put a portion of it at the disposal of local authorities along with Haffkine's, and they would be allowed to choose which they would have. We should like to see it, of course, tested alongside with Haffkine's.

4370. You want to work out your old stock of Haffkine's before you work this new prophylactic?—No, not exactly that; one has, however, considerable misgivings that the Haffkine remedy will not keep indefinitely. I find myself compelled to test it every few months to see that it has lost none of its virtue. Some of that which we made a good many years ago we found on subsequently testing it later was not as potent as it had been, and that we discarded. On the other hand, we found that some of equal age was as potent as before.

4371. In short, you do not really think much of your Haffkine stuff?—It was the best thing to be had for a very long time.

4372. You thought it was?—And in India they consider that it has had a very considerable effect in saving life.

4373. But now you do not think much of it?—No, I would not say that. I should use it now if we had plague in this country.

4374. Sooner than the other?—Personally, of course, I should prefer the other. If I had to be inoculated against plague, I should prefer the new remedy, but I would not force it on everybody until we know more about it.

4375. (Sir William Church.) I think the Commission hardly appreciate, perhaps, the position of the Local Government Board, judging by some questions which you have been asked. It is no part of the duty of the Local Government Board, is it, to try to discover remedies?—No, it is not our business to discover remedies.

4376. What you do is you endeavour to find out from the reports of your inspectors what is the effect of remedies which have been put upon the market, so that you may recommend that those which appear to have value should be used, and not those which have none?—Very largely; but at the same time, we should, perhaps, make some original investigation towards getting a preventive. For instance, I think it is quite possible that the Board might engage in some such investigations in regard of cerebro-spinal fever, which is now apprehended as coming upon us. Probably there will be a great demand before long for a prophylactic and for remedies for the disease, and it is quite possible that a good many very doubtful things may be put upon the market, and we probably shall have to test them, and while we are doing that, we might make some investigations of our own to see whether we could make anything of any value as a remedy.

4377. Just following up what Lord Selby asked you, the gentlemen whom you employ to do special work are completely under the control of the Home Office?—Completely, just as any other workers would be.

4378. Those whom you nominate to do that work

have to apply for a licence in exactly the same manner as any others; that is to say, their names go before the two persons who are authorised under the Act to sign an application for a licence just as in the case of anybody else?—Yes; and so with the certificates. I am bound to say that some of them demur rather to that; they think that if they have the authority of the President of the Local Government Board to undertake the work, the Home Office should at once assent to their doing it; and if you were to have members of the staff who work for us before you, they might perhaps advance some such suggestion as that. But we ourselves, so far as the Board are concerned, are perfectly content with the supervision of the Home Office; it is done very well indeed, so far as we can see, and we have no reason to object to it in any way. But the individual workers do.

4379. They have to make a return of all the experiments they make upon animals to the Home Office as well as a scientific report to you?—Yes, they make a numerical return to the Home Office, and they furnish a detailed account of their experiments to us.

4380. Would you tell me, speaking with regard to the use of experimentation on animals as a means of diagnosis in the interests of public health—I will put what I want to get at more directly—for instance, at the present time has a port sanitary authority any means of diagnosis so sure as that of experimentation on animals with regard, say, to plague?—No, nor with regard to cholera.

4381. But with regard to plague they have not?—No, and it is essential that they should get as speedily as possible a definite identification or the reverse of the disease, because in the meantime a ship which has brought the disease, perhaps, may be hung up and trade be delayed and commerce damaged, so that not only do they want to know accurately whether it is that particular disease or not, but they want to know very promptly.

4382. And the surest and most accurate means that you are at present acquainted with is inoculation of animals with the suspected fluid from suspected cases of the disease?—Yes, in order to be absolutely sure both for plague and cholera you have to do that I think at present—certainly for plague.

4383. I need not go into the success or want of success which has followed some of these remedies which have been lately introduced, such as the serum treatment; but have you any hopes of any method of preventing disease being found out which will be more successful than the serum treatments which are now made use of in the prevention of disease?—Artificial immunisation is, as it were, in its infancy. What one looks to in the future is to find some means of protecting the human body by the inoculation of materials such as serum or what not, and the essential thing will be, of course, to secure something which is a lasting protection in that way. At the present time a good many of these preventives are perhaps a little transient as regards immunity conferred by them. The hope of the future is that we shall get something which will not only protect for the present but will protect almost indefinitely.

4384. Referring to an answer to a question by Colonel Lockwood, could you tell us at all what used to be the percentage of deaths, say, fifteen years ago in the Metropolitan Asylums Board's hospitals from diphtheria?—From 25 to 30 per cent., I should think. I should have to check the figure.

4385. I have some statistics here. For instance, the case mortality in the London hospitals from diphtheria in 1889, which is the first year, I think, for which the statistics are quite complete, was 40 per cent.; at the present time the deaths are 12 per cent. since the introduction of antitoxin. Should you say that any alterations in the general sanitary condition of the people would account for so great a fall in the mortality as that?—No, there is no modification of sanitary conditions at all parallel with it.

4386. You yourself would ascribe it to the use of antitoxin?—I have not any doubt whatever that it is due to the use of the diphtheria antitoxin in those hospitals.

4387. It is often said by those who think it is of no use that the general mortality from diphtheria has not fallen; at all events, that it has not fallen to anything like the same point as that. Have you any views that you can give the Commission as to why the

total mortality in the country has not fallen more, considering how greatly the mortality has fallen in the Asylums Board's hospital?—That is a very difficult question. But of course the use of diphtheria antitoxin is not yet universal.

4388. It is, but I thought you might be able to give us some explanation?—It might, of course, be inferred that there would have been a very much larger mortality but for the use of this antitoxin. That, however, is mere surmise.

4389. Do not you think that the present laws with regard to education have a good deal to do with keeping up diphtheria in the country?—I think at one time there was perhaps a disregard of the necessity of keeping children at home when they had an anomalous sore throat which might be diphtheria, but I think there is a good deal more care taken now than there was even a few years ago, and it is more universally done.

4390. Putting it in other words, do not you think that the more general attendance of children at school, and massing them together in schools—I mean that fewer children escape having to go to school than used to do—has had a good deal to do with keeping up the large amount of diphtheria present in the country as compared with the period before compulsory education?—That would be comparing the state of things now with that before 1870. I do not think we have got sufficient data to compare the two periods. I would not like to say that in the present day, at all events, the aggregation of children in schools has very largely conduced to the maintenance of diphtheria mortality.

4391. Nor with keeping up the disease in the country?—Nor with keeping up the disease in the country. One believes, of course, that it is very largely kept up by personal relations, and that it still continues to prevail through neglect or want of recognition of the minor and anomalous cases. Whether such cases are proportionately more common now than they were a generation ago I cannot tell.

4392. Is there any evidence to show that diphtheria is transmitted in any other way except from person to person?—It has been disseminated in milk on a good many occasions; not by water, certainly. Insanitary circumstances appear to have very much less to do with the dissemination and maintenance of diphtheria than is the case with many other diseases. Diphtheria seems to be, I might say, almost independent of them.

4393. Therefore, so far as we know at present you would agree that the organism which causes diphtheria is not apparently propagated in the soil?—Apparently it is not.

4394. (*Chairman.*) When you say it might come through milk do you mean that the animal from which the milk was drawn might be a diphtheritic animal?—It is a moot point whether the animal itself supplies the infection or whether infection simply gets accidentally introduced into the milk after it is removed from the cow; but however that may be, the disease is sometimes disseminated by milk on a very large scale.

4395. (*Sir William Collins.*) As regards diphtheria in schools, to which your attention has been directed, has it often been the case that the observation of the children in our public elementary schools has been the means of detecting the presence of diphtheria?—I should think that in later years that has had a considerable effect in securing control of the disease. The school teachers, I think, are much more alive and alert than they were to possible infectious disease, and I think there is a greater disposition amongst sanitary authorities to invite the assistance of the medical officer of health, or, at any rate, medical advice, in looking after the children in schools; and I should think diphtheria has been identified much more of late years than it used to be amongst persons of the age involving school attendance.

4396. And such identification may have led to prevention of spread, I suppose?—If the proper precautions are taken it should, of course, prevent the spread from person to person.

4397. In the case of London do you think that such precautions are taken?—I know very little of London; my knowledge is only that of the ordinary reader of reports. We have very little to do with the administration in London, but I should be disposed to think that as compared with the supervision and alertness

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of years ago that of to-day ought to be tending to reduce the amount of diphtheria in London.

4398. You were asked as to the case mortality statistics of diphtheria prior to the use of the antitoxin and subsequently to its use. What were the means possessed by the profession of diagnosing diphtheria before the date of the antitoxin?—Purely clinical observation for the most part.

4399. Should I be right in thinking that since the introduction of the antitoxin the diagnosis of diphtheria has been largely upon bacillary grounds?—Yes, certainly.

4400-1. Will that have the effect of diagnosing cases of diphtheria which previously would have escaped?—I think so. There is something in that, I think.

4402. So that a considerable number of cases of diphtheria would now be diagnosed by the bacillary test which would not have been so diagnosed by clinical observation?—They would not have been notified.

4403. Would that have any effect upon the case mortality?—It would produce, of course, a large number of cases on which to rate the absolute deaths, and to that extent it might have the effect of giving a seeming reduction of the case mortality which was not altogether real. That would have to be taken into consideration in judging the case mortality now as compared with the case mortality of years ago.

4404. It would be a factor to be considered possibly with regard to the point that was put to you as to the total mortality rate not having fallen while the case mortality had fallen?—Yes; the mortality might be stationary, while identifications of the disease were becoming more abundant.

4405. I think you referred to Sir John Simon as being the first to advise a Government Health Department to utilise vivisection experiments for purposes of scientific investigation?—Yes; it commenced under his reign.

4406. Could you tell us in what year?—I think it was about 1864 when he first mentioned it. I have a note here which perhaps might be of assistance in this matter. I see that Sir John Simon, in his "English Sanitary Institutions," says: "Soon afterwards, *i.e.*, after 1869, the departmental organisation was strengthened in an important outwork, the first beginnings of which, five years previously, *i.e.*, 1865, had been noticed in my eighth annual report."

4407. That would have been when he was Medical Officer to the Privy Council?—Yes.

4408. And then from 1865 until the present time the Privy Council or the Local Government Board has had resort to these vivisection experiments for the purpose of scientific investigation?—Yes, systematically, since 1870, when there was a special grant from the Treasury for the purpose of scientific investigation.

4409. Could you give me an idea as to the amount of those grants?—Yes. Formerly, when it was first granted, I think under Mr. W. E. Forster's dispensation, in 1870 or 1871, the sum was £2,000; and it remained at £2,000 for a good many years, until, I think, soon after Sir John Simon retired, when it became £1,900, and £1,900 it has remained ever since.

4410. Then for the last thirty-six years we may take it that it has averaged £2,000 or £1,900 a year?—Yes.

4411. Did I correctly understand you to say that at one time the Local Government Board had laboratories of their own?—They hired a laboratory, and paid directly for the use of the laboratory; it was the St. Bartholomew's Hospital Laboratory. They paid some couple of hundred pounds a year. I think they paid for the laboratory, and attendants, and re-agents, and things of that kind; but in more recent years that has been discontinued, and now each worker makes his own arrangements as to the use of a laboratory—of course it has to be a licensed place—and his remuneration takes account of the expenses that he is put to in that way.

4412. Do the Board ever employ a whole time bacteriologist?—No, they have not done so.

4413. Would you give us the names of the scientific authorities who have been employed from as far back as you can remember until the present time?—Dr. Andrewes, Dr. Blaxall, Dr. Theodore Cash, Dr. Edmund Cautley (I am giving them alphabetically), Dr. Cory, Dr. Creighton, Mr. Dowdeswell, Dr. Dupré, Dr. Feneage Gibbs, Dr. Mervyn Gordon, Dr. W. S. Green-

field, Dr. Grunbaum, Dr. Haldane, Dr. Hamer, Dr. V. D. Harris, Mr. (now Sir) Victor Horsley, Dr. A. C. Houston, Dr. A. A. Kanthack, Dr. E. Klein, Mr. Parry Laws, Mr. Alfred Lingard, Dr. MacFadyen, Dr. Sidney Martin, Dr. Burdon Sanderson, Dr. W. R. Smith, Dr. Thudichum, Dr. John Wade, and Dr. Wooldridge.

4414. And would you be so good as to give me the names of those who hold letters of engagement now?—Dr. Klein, Dr. Gordon, Dr. Grünbaum, Dr. Sidney Martin, Dr. Horder, Dr. Savage, and Dr. F. W. Andrewes. I think that is all.

4415. Do you say that Dr. Klein is still employed?—He is still employed.

4416. You told us that you issued a special note to each experimenter that no painful experiments were to be performed without anaesthetics?—Yes.

4417. And that they were to report such to you?—Quite so.

4418. Has your attention been called to the evidence given before the former Royal Commission by Dr. Klein?—Of course I, in a sense, know it well. I remember hearing of it. I was a junior at the time.

4419. On page 183 I see the question was put to him, "Then for your own purposes you disregard entirely the question of the suffering of the animal in performing a painful experiment?" And his answer was, "I do. Did that evidence have any effect upon you as regards this note that you issue?—I think very possibly it had a good deal to do with the origination of Mr. Forster's wishing that each one of the investigators should be specially cautioned against engaging in painful experiments if they could possibly be avoided. I have already quoted a memorandum made upon the subject by Mr. W. E. Forster to Sir John Simon.

4420. What was the date of Mr. Forster's memorandum?—February 17, 1874.

4421. The evidence of Dr. Klein to which I have called your attention was given on October 23, 1875, according to this Blue Book?—The date of the minute by Mr. Forster is February 17, 1874, and I suppose that thereafter Sir John Simon began to give the special caution that is now given. When exactly he began it I could not say.

4422. The evidence of Dr. Klein could hardly have been the occasion of the minute by Mr. Forster?—No, it looks as if it were not so; I agree.

4423. In his evidence before the former Commission Sir John Simon stated that it was his aim to obtain exact scientific knowledge of the causes and mode of acquiring of any disease which is in question. I suppose that is still the aim?—That is still the aim.

4424. And in your own *précis* I see that you state that your object is to secure immediate administrative advantage?—Quite so; one of our objects.

4425. What would you regard as the most typical or most successful example of these researches culminating in exact scientific knowledge or in securing immediate administrative advantage?—It is hard to point to anything that has given immediate advantage. Some instances of recent work in scientific research, say within the last ten or twelve years, leading to important results in regard to disease prevention or public health administration, are with regard to intestinal micro-organisms, the lead-poisoning abilities of water supply, bacteriology of scarlet fever and diphtheria, and the preparation of Haffkine's and other prophylactics. For instance, in regard to presence and abundance of particular intestinal organisms, say in drinking water, a guide has been afforded to administrative authorities and their expert officers with regard to the uses and limitations of bacteriology in these matters. A series of reports has been made available when a question arises as to the bacteriological methods to be adopted or the interpretation of bacteriological results. There can be no doubt that these reports have been and are being used and studied, to the advantage of good public health administration.

4426. Is that what you would cite as an example of immediate administrative advantage?—I am speaking in a general way. An immediate administrative advantage, of course, is the detection of imported infectious epidemic disease which may spread over the country, as, for instance, plague, or cholera, or cerebro-spinal fever, and matters of that kind.

4427. Are you satisfied that you have the means of certainly identifying the presence of cholera by micro-

organisms?—Yes, I think that the Koch's comma is generally accepted—at any rate, that is the gauge by which it is universally tested throughout this country and throughout Europe. I should like to say that if it is wished that I should give a definite statement of how one thing has led to another for administrative benefit, I could say a good deal as to the question of disinfection on board ship. It would take up rather the time of the Commission, but we have gone on there from one point to another, attaining in the end definitely useful results.

4428. (*Sir William Collins.*) I think you told me that the Local Government Board were satisfied that the diagnosis of cholera could be made upon the finding of the Koch's comma bacillus?—Yes.

4429. Did Dr. Klein dispute that position as to the diagnostic value of the Koch's comma bacillus?—He entertained doubts at one time, more especially after his visit to India, I think, as to whether the Koch's comma bacillus was *per se* the one agent; but afterwards, on further investigation, he fully accepted, I think, Koch's dictum that it was the essential cause of cholera.

4430. Did he not state that he had partaken of it with impunity?—Yes, but I do not know that that proves very much one way or the other.

4431. That was during his sceptical period I suppose. In your *précis* you allude to the serum used against plague and enteric fever, and you say that neither has proved altogether satisfactory?—That is so.

4432. Would you kindly amplify that? In what way have they proved unsatisfactory?—Allegations have been made that the sera of both enteric fever and plague are not so completely protective as is to be desired; that notwithstanding that people are injected with the prophylactic certain of them nevertheless take enteric fever or plague, as it may be, and die, and that it is therefore desirable, if something which is more fully protective or more lastingly protective could be discovered, that it should be put at the disposal of persons exposed to the one or the other disease.

4433. Does the Local Government Board recommend Wright's anti-typhoid serum?—No, we have never used it. You will remember that its value was disputed among the military authorities in South Africa, and they have appointed a committee I think of the Army Medical Board now sitting with a view to elaborate a more satisfactory serum; and since they have undertaken that we have not of course touched the matter.

4434. Does the Local Government Board discountenance the use of Wright's anti-typhoid serum?—No. We do not supply it, and we have not given the public authorities any special facilities about it. They are quite at liberty to use it, but there would be a question of course as to payment for the use of it. That is what they would have to come to the Local Government Board for.

4435. Then besides these sera not proving protective against attack or death have they been unsatisfactory in regard to untoward results?—Not in this country, certainly. I will not be very sure as to other countries. There was some tetanus, of course, in relation to the plague prophylactic in India, and I think there was something similar in the United States. I am not sure that it was not in relation to the diphtheria antitoxin. But the results as a rule have not been unsatisfactory. There has been no serious damage or death, so far as I know, resulting from a very free use indeed of the prophylactics. But I am speaking rather of our own experience in this country. None of these sera except the diphtheria antitoxin have been very largely used here, and the diphtheria antitoxin has not been so largely used as perhaps it might have been.

4436. Were the tetanus cases in India to which you refer in connection with the plague serum fatal?—I think every one of them—some 18 or 19. It was a disputed point as to when it was that the tetanus bacillus got into the particular phial of the prophylactic; that I think has been the subject of inquiry by the Indian Government.

4437. Do I correctly understand that the Local Government Board recommend the use of sera in the case of cholera, plague, typhoid and diphtheria?—I do not think they go so far as to recommend, but they are willing, as I was saying, to place some of these

sera at the disposal of the authorities to a limited extent, to deal that is with the first beginnings of epidemics; and they facilitate the purchase and use of them by allowing the expenditure. We have had very little to do with the anti-typhoid prophylactic, and I will not be quite sure that we have ever had an application to the Board to sanction expenditure for that purpose; we certainly have not pressed it officially.

4438. Are these sera prepared under the authority of the Board?—The sera that we issue are.

4439. Do you sell them?—No, we give them away; we put them at the disposal of local authorities; therefore it is only to a limited extent that we issue them. Chiefly it has been of course as regards diphtheria and plague to these authorities. The diphtheria antitoxin the Board put more largely at the disposal of the country generally; but there was no very large demand for it, and the Board no longer make the diphtheria antitoxin with the view of putting it at the disposal of the authorities.

4440. Does the Board guarantee the sera that it authorises as being free from possible harmful results?—They give no absolute guarantee, but they take every precaution so far as they can that the material shall be quite innocent, except for the purpose for which it is intended.

4441. You spoke of there being considerable misgiving with regard to Haffkine's serum?—There was. It contained an addition of 0.5 per cent. of carbolic acid to which some people objected, I believe simply because carbolic acid poisoning has been occasioned by it; and one of our objects when we began to make the plague prophylactic was to do if we could without carbolic acid.

4442. And the new remedy which the Board are thinking of putting on the market against plague I understand was to be prepared from the dead tissue of a plague patient?—Not to put it on the market, nor from the plague patient; the dead tissues of the plague-rodent are in question, and this material will be made available for local authorities in the same way as the others for those who desire to use it; they would not be charged anything for it.

4443. Has it not been used on human beings?—No, not in this country.

4444. Only on the rat?—The rat and the monkey.

4445. Are you able to say for how long the prophylactic influence lasted on the rat or the monkey?—That has to be a matter of observation, of course. In the case of the rat I believe it lasted a number of weeks. In the monkey it has not yet been tested for so long.

4446. Are you prepared now to recommend it for the human being?—I am not very sure about it, I should have to consider it, and I myself should like some further experiments made first.

4447. Have you found at all that what may prove to be a success as regards prophylactic in the laboratory has failed when it has been made a matter of application in the ordinary use?—I suspect that the degree of success in protection that is obtained in the laboratory is not to be confidently anticipated when the prophylactic is applied to the human being. I am afraid it might be a good deal less.

4448. Would you state on what grounds you make that differentiation between the state of affairs in the laboratory and that in general use?—Because in the laboratory you can contrive your conditions and make them tolerably exact and repeat and vary them from time to time, but unfortunately when you come to nature and the human being the conditions are very different, and you do not always have the conditions which suit the material that you are using.

4449. Does the Local Government Board recommend the use of the diphtheria antitoxin as a prophylactic?—Yes, and we made it on a considerable scale a few years ago and put it at the disposal of the authorities.

4450. Is it used?—No, the demand for it entirely fell off, and naturally the Treasury did not see why we should go on making it for no particular use, and it was discontinued. We had a special grant from the Treasury for part of the cost for making it.

4451. Have you considered the question of advising the Board to seek power to compel its use?—No, we have not gone as far as that.

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4452. I think you stated that there are certain diseases which can be identified without recourse to experiment on living animals. Which are those diseases which come within your purview?—I said rather that I was hoping that we should be able, by differentiation according to culture behaviour, to identify diseases without resort to the animal body. I was thinking of diphtheria and of cerebro-spinal fever—the meningo-coccus.

4453. You mean by finding a definite specific organism?—By finding a definite organism which will react always in a certain way to a certain series of culture tests. It is morphologically tested and as to the temperature at which it grows and that at which it dies; and in that way, if one's hopes were fulfilled, one would be able to do away with a great many animal experiments. As matters stand, a great many authorities cannot resort to the Department, or a good deal of delay is liable to arise; it would require a great many more licences if the practice were generally adopted to test every case that arose in animal bodies. We have no authority to formulate a standard test, apart from the animal test, if we discovered one; but if we found what seemed to be a suitable and trustworthy cultural test, we should make it known and indirectly recommend it for adoption by local authorities, no doubt.

4454. Let us see in which of these diseases we have been able, in your opinion, to establish such a test?—I cannot say that we have got to that stage with any one of them. I am hopeful that we are rather near a satisfactory cultural series of tests with the meningo-coccus. There are morphological and cultural tests for the diphtheria bacillus, but there are so many kinds of diphtheroid bacilli that cultural tests of them, though all very well, perhaps, as regards positive results, do not, I think, as regards negative result, lead to definite conclusion without a test on the animal body.

4455. What attitude has your Board taken up with regard to tuberculosis, both as regards diagnosis and as regards prophylactic, or the treatment by serum?—The Board is waiting for the Report of the Royal Commission on Tuberculosis, I think. Whether in view of the Report which has been recently issued, and of further Report to follow, the Board will see their way to any legislation or not I do not know.

4456. Have you had an opportunity of seeing the Report which has just been issued?—Yes, I have glanced through the Report that has been recently issued; but the Board, of course, as a Board, have not considered it—the officials of the Board have not considered it.

4457. Do you accept the position that Koch has taken up, as regards the bacillus, as the cause of the tubercle?—Yes; I believe that the bacillus of tubercle is the cause of tuberculosis.

4458. Were there not investigations under your Board, or under your predecessor, by Sir John Burdon Sanderson, indicating that tubercle was not a specific disease, but could be built up from a common septic ferment?—I do not remember it of tubercle. There are some of the earlier lectures of Sir John Burdon Sanderson which are capable of being read in more than one way. I do not remember it in that connection. He gave a lot of evidence on the building up of increased infectivity of septic matter in passing from animal to animal, but I do not remember that he mentioned tubercle, as it were, in that way, or that he professed to do so.

4459. Do you remember his reporting that in rodent animals the tubercular process may originate not only by inoculation of tubercle but by any irritation of requisite intensity applied to the subcutaneous tissue?—I think, in the days of Villemin and Wilson Fox, it was affirmed that it might be done by any irritant as, for instance, indiarubber; but in those days they had not got the check since supplied by the discovery in the tissues of the tuberculosis bacillus, and therefore to that extent those experiments would not go for much now.

4460. But was it not stated at the time—1868—that the truth of this inference has been completely established by the experiments of Dr. Wilson Fox, Dr. Kile, and others?—As to the transmission of tubercular matter setting up of tuberculosis, *i.e.* by inoculating caseous tubercle, that was established by the experiments; but that anything else than tuberculous matter would do it, I do not think Dr. Wilson Fox said that. I thought he was rather the other way.

4461. The statement I read to you was in reference to the allegation that tuberculous inoculation could no longer be regarded as dependent upon inoculated material having been taken from a tubercular individual?—Is that from Sir John Simon?

4462. These are Sir John Burdon Sanderson's and Dr. Wilson Fox's experiments?—I expect Sir John Burdon Sanderson modified his view very much before his death. I do not think he would have said that in his later days.

4463. Probably you do not remember, then, that Sir John Simon, in his preliminary report, called attention to the value of those experiments?—No, I do not; that is rather before my day; it is 30 years ago or more.

4464. But an established fact is an established fact, I suppose, whether it is 30 years ago or later?—What I mean is that my acquaintance with this is necessarily less perfect than with recent affairs.

4465. Has your Department, the Local Government Board, come to any decision as to whether tuberculin is a cure for consumption?—No, they have not entered into the question.

4466. Have they not investigated the question?—Yes, they have investigated the question in reference to the effect of tuberculin on animals, but they have not considered the question of its application as a cure for tuberculosis in a human being.

4467. Do you mean that among the many scientific investigations instituted by the Local Government Board they have not thought it worth while to consider the question of the cure for consumption as advocated by Koch?—I believe they have had some experiments made on the lower animals, but I do not know that the result of those has encouraged them to recommend its use in the human being.

4468. Have you come across any evidence which would confirm the statement of Koch in his book called "The Cure of Consumption," where he states that "phthisis in its early stages has been cured with certainty by this remedy"?—The use of the tuberculin cure for phthisis throughout Europe fell, after first advocacy of it, into desuetude for a good many years, but I believe there are some physicians now in this country who are rather disposed to recommend the use of tuberculin, or some matter rather closely allied to it, as not without advantage in early cases of phthisis; but I know nothing beyond what I have seen written on the subject.

4469. The words that I put to you from Koch are "Phthisis in its earlier stages has been cured with certainty by this remedy"?—I think it was anticipated at first that not only the early disease, but almost any stage of it might be curable by tuberculin. I remember that at the time when the cure was first promulgated people flocked to Berlin in enormous numbers to undergo the tuberculin cure, and with very unhappy results in a good many cases; they were a good deal worse for the treatment.

4470. My point is whether that statement is to be held to be true now that "phthisis in its early stages has been cured with certainty by this remedy"?—I do not know. We have not to do with the cure of it so much as the prevention of it, and I cannot say that I have considered the question of cure with the view of advising the Board.

4471. Does the Local Government Board recommend it with a view to prevention?—The Board has not done so.

4472. Has it been found by the Royal Commission which has just reported that the tuberculin test for consumption in monkeys is unreliable?—I had not noticed that, but I believe that they consider it is of extreme use with the bovine animal.

4473. Has the Local Government Board investigated many diseases of the bovine animal?—Not in recent years. In Sir John Simon's day they were doing a good deal, and in Sir George Buchanan's day, but I do not remember anything being done with the bovine animal in later years since there has been a Board of Agriculture established.

4474. Was it suggested by the researches carried out under your Board that a cow could suffer from diphtheria and scarlet fever?—Yes, it was.

4475. In the shape of a specific form of eruption?—In the shape that the particular malady from which the animal suffered, which was supposed to be

analogous to diphtheria, was accompanied by udder eruption in many cases.

4476. There was a good deal of investigation of udder eruptions at one time, was there not?—Yes, a great deal.

4477. I see in the Report of the first Commission on Vivisection it was stated that it was in experiments upon cows that the original cow-pox, a disease stated to be derived from grease in the horse, was ascertained; is it held by the Local Government Board that cow-pox is derived from grease in the horse?—I do not know that the Board hold anything very definitely on the subject. They are concerned with the vaccine that they manufacture and distribute, testing it always through a great series of calves before they issue it to public vaccinators.

4478. You speak of the manufacture of vaccine by the Local Government Board. From what source is the Local Government Board lymph derived?—When it was started, I believe it was obtained in the first instance from Cologne, but what was the exact source of it in Cologne is not very certain; it was alleged at the time that it was from casual cow-pox. Since that day, however, our lymph has been renewed again and again, and always from the Continent, and what the remote source of any vaccine from the Continent is now, I should think it is almost impossible to say; probably a good deal of it is variolous.

4479. A good deal of it is derived from human small-pox?—I think so. I do not think there is any concealment about it on the Continent. The origin is variola, more or less remote, I think.

4480. In the case of those bodies who use it to-day under the auspices of the Local Government Board, is it derived originally from human small-pox?—You would have to call Dr. Blaxall to know exactly about these matters, but it is very largely still simply Cologne lymph, and that lymph has only been kept going, I understand, by frequent retro-vaccination from human bodies to calves. All the lymphs, I am given to understand, that have a direct so-called cow-pox origin are liable to become inactive, and to require constant retro-vaccination, and even then they are found to be not altogether satisfactory. I am given to understand that on the Continent it has become more and more the custom to use lymph of more or less remote variolous origin, derived certainly by the direct or indirect implantation of variolous matter in the bovine animal.

4481. Then the direct origin of most of the Local Government Board lymph at the present time is Continental?—Cologne, I believe. I cannot say what proportion is now Cologne. It was believed at the time when we adopted the Cologne source that we were going to get the natural cow-pox to start with, and none satisfactory was forthcoming at home. I believe at the particular time there was not any discoverable in this country. I, of course, was not in the office then, but I believe that Cologne lymph was taken as representing most nearly at that day current lymph arisen from the casual cow-pox.

4482. Then so far as you know the remote origin of Continental or Cologne lymph it is likely to be variolous?—No, so far as I know we were led to believe that the Cologne lymph was from casual cow-pox. If casual cow-pox arises only from a variolous source then it was variolous, but I do not know whether that is so. Cow-pox is almost a generic term, and what particular cow-pox started the Cologne lymph I do not know, but Dr. Blaxall, of course, has had to study these things; he is more directly responsible than I am for the lymph that is used by us, and he would be the best person to inquire of as to the remote source of any of the lymph that is at present in use by the Local Government Board.

4483. But apart from casual cow-pox, have there not been experiments both in this country under your Board and abroad with a view to inoculate the cow or calf with human small-pox?—In most countries there have been such experiments. We have had no experiments in the Local Government Board for a good many years on that subject, but they have been made in this country and in other countries very abundantly.

4484. Did not Dr. Klein report to the Local Government Board some years ago the results of his inoculations?—Yes, and they were not satisfactory.

4485. They were mostly negative?—Yes, that would be about 15 years ago at least.

4486. Did not Chauveau, in France, practice inoculation of small-pox into the horse and cow?—Yes, he did.

4487. Did he produce an outbreak of small-pox by the use of that lymph?—I do not remember.

4488. Are you able to say that the lymph now in use is not of variolous origin?—No, I could not say that. All I can say is that the lymph we use is passed by a great many removes, and with perfectly satisfactory results so far as the anatomical appearances are concerned, through a series of calves before we allow it to pass into circulation. It reacts uniformly effectively on the calf. For instance we should not think of allowing to be used in the public service a lymph which was admittedly only recently derived from a variolous source, whether directly from the human being or through a monkey, and then on the calf. If we were driven to it we should certainly require it to be passed through a great many removes in the calf before we thought of adopting it.

4489. Did not one of your own medical inspectors, Dr. Copeman, make some observations upon the subject?—Yes, but not as an officer of the Board; entirely as a private individual and in his leisure time.

4490. Did he endeavour to raise a stock of vaccine lymph by putting through a certain process scabs derived from small-pox patients?—I think he did. He tried a variety of ways; passing it intermediately through monkeys as well as directly in the calf.

4491. Was that lymph used upon children?—Not by the Board. No lymph that Dr. Copeman produced has been used either for our calf vaccination or for our infant vaccination.

4492. Was it used for the vaccination of children?—Not by us.

4493. Are you able to say whether it was or was not used for the vaccination of children?—I cannot say what Dr. Copeman did with it; you must ask him. He did not try it on any children for whom we were responsible.

4494. You do not know whether it was used upon children?—I think that ultimately it probably was, but not with our authority.

4495. (*Sir Mackenzie Chalmers.*) I did not quite understand one of your answers. Does Dr. Klein now doubt that the comma bacillus is distinctive of cholera?—No, he has accepted it entirely, and works by the comma bacillus, so to speak, in the identification of cholera; it is a good many years since he finally accepted the comma bacillus as distinctive of cholera.

4496. For some years he has accepted it?—Yes. It was as far back as the eighties I think that the controversy arose as to whether the comma bacillus was the real Simon Pure.

4497. Are there any competent authorities who still entertain the same doubt that Dr. Klein entertained then?—I do not think that there are, except possibly there might be some persons who have hesitation in accepting a single infection; some may think cholera due to a mixed infection; that the common bacillus would operate more efficiently along with something else than it would ever be able to do simply by itself. That question may be seething in people's minds now.

4498. But it is a distinctive test?—True cholera does not occur in this country without the comma bacillus; that is the line on which it is worked now.

4499. It is a distinctive test in this country, though not necessarily an efficient cause. Is that what you mean?—Koch's comma is always associated with Asiatic cholera cases.

4500. Do you happen to know as regards the plague serum which was used in the Punjab where certain tetanus cases resulted whether Dr. Haffkine has given any explanation of it?—I believe that he has. I believe there are two theories in explanation, one of which is that it was infected at the laboratory, and the other that it was infected at the place of destination where it was used, but as to what the verdict is I do not know.

4501. You state that you are willing to supply the diphtheria antitoxin as a prophylactic?—Yes, we were.

4502. Was that simply for contacts—for people who have been in contact with diphtheria or for nurses or for what purpose?—We supplied it and put it at the disposal of the authorities mainly of course as

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a prophylactic, that being our special business as regards preventive medicine, but we should not object to its being used curatively, and no doubt it was used curatively by a good many people. But the demand for it fell off, as I was saying, to nothing.

4503. Was it your idea that it should be used for people in contact or that it should be used generally in case of epidemics, or what was your idea?—That it should be used generally only for persons in contact; if an authority for instance desired that all children attending a given country school should, when diphtheria occurred throughout the parish, have an opportunity of being injected with it. I do not think the Board would stand in the way of a local authority using the diphtheria antitoxin for such purpose.

4504. Have you any idea for how long that prophylaxis would last?—As a prophylactic?

4505. Yes?—It is surmised that it would not have any very long duration. I do not know exactly upon what grounds. I do not think there is much evidence of these people after being inoculated with the prophylactic taking diphtheria later on elsewhere. The chances are perhaps that they were not again exposed to it. There is not sufficient evidence of exposure after long interval of people once inoculated and protected by the diphtheria antitoxin to judge of its value, but in the immediate presence of diphtheria and for some days or weeks possibly it is efficacious in such cases.

4506. It is efficacious for some days or weeks?—Yes. It seems to be so. In the particular epidemic to which persons inoculated are exposed it seems to be satisfactory.

4507. You do not recommend its use for nurses in diphtheria wards?—I think it is commonly used by the hospital authorities. At any rate we should not have anything to do with that; it would be a case, so to speak, of domestic management.

4508. Do you still manufacture it for curative purposes?—No, we do not now manufacture the diphtheria antitoxin; there is any amount of it in the market now of very high strength, and it can be generally purchased. We did manufacture it in the early days before it was generally available. The Board had a misgiving that a great deal of that which was being imported from the Continent was poor, of uncertain quality, or that it was simply coming here for the market, and therefore they felt it incumbent upon them to make available a material of a kind which was more trustworthy.

4509. Is it part of the duty of the Board to test what comes over from abroad—to sample it in any way, so as to see whether it is pure and of proper strength?—No, there is no test of imported prophylactics or of curative agents by the Board certainly, or so far as I know by any other Government authority.

4510. So that the general practitioner has to take it on trust?—The general practitioner has to take it on the reputation of the firm from which he obtains it.

4511. I did not quite understand your evidence as regards Burdon Sanderson's experiment on the production of tuberculosis. I think Sir William Collins said they were in 1868?—Yes, they were a great many years ago.

4512. Have further advances and further experiments been made since then?—Of course the experiments now made are not only with the crude tubercle but with the tubercle bacillus itself. I believe it is possible in inoculating with the tubercle bacillus to get not only disseminations throughout the body which contain tubercle, but disseminations or sparse formations of material which does not contain tubercle in the sense that some of them do not always set up tubercle in the animal into which they are inoculated; so that it is possible to get a nidus without acutely tubercle bacillus in it; at least that is the way in which I read it. It would require a good deal of further consideration on my part before I gave a definite opinion upon that subject.

4513. You said that the lymph obtained from the Local Government Board is carefully tested before it is issued to the public?—Yes, it is certainly. It is kept under glycerine and tested from time to time in plate culture to see that nothing grows on the nutrient medium in the way of extraneous organisms.

4514. (*Colonel Lockwood.*) Do the Board guarantee the vaccine?—No, they do not guarantee it, but they issue it on the understanding that it is the best quality lymph so far as they are able to secure this.

4515. (*Sir Mackenzie Chalmers.*) Are public vaccinators allowed to use private lymph or are they confined to Government lymph?—They are expected to use Government lymph, and they are certainly bound to use it if the patient desires it. They may use other calf lymph, but they must not use arm-to-arm lymph so long as they are public vaccinators.

4516. Arm-to-arm lymph is prohibited?—Arm-to-arm lymph is prohibited to public vaccinators.

4517. (*Mr. Ram.*) With regard to the operations that are undertaken at the desire of the Board and for the Board, you say that it is no part of your business to superintend them or to see that the Regulations of the Act are observed?—It is no part of my duty to personally supervise the scientific workers employed by the Board in carrying out their investigations.

4518. So I gathered. Therefore, to whatever extent they have to be looked after, to that extent you rely upon the Home Office to do it?—Entirely.

4519. Are you aware of the extent to which the Home Office inspection is carried?—I believe that the Home Office Inspector visits them at irregular and uncertain intervals, without notice, some three or four times a year.

4520. That is what the evidence has been?—Yes, and, of course, if he is interested in a particular line of inquiry he might go more often.

4521. But, although it is no part of the Board's duty or of your duty as their Medical Officer to look after the operations, you have sent out this instruction which you have handed to us?—Yes, in which they are definitely warned not to undertake painful experiments without due ground for it, and to let the Medical Officer of the Board know if they are proposing to undertake such experiments, the fact being that it is understood amongst the men who work for us that in ordinary course the only animal experiments that they will make will be feeding experiments and inoculation experiments.

4522. This instruction is taken verbatim from the concluding words of Mr. Forster's memorandum?—I believe it is in those words.

4523. As the result of this instruction, do you find that you get a report from time to time explaining the object of any experiment?—Yes, I have had two, and I think both of them were from Dr. Sidney Martin, stating that he wished to try the effect of a material which he had got as to whether it influenced the blood pressure or not. In that case, of course, he had to give an anæsthetic, and he had to kill the animal before it recovered from the anæsthetic. He did proceed in that way, and he reported on it fully.

4524. Are those the only two reports that you have received in six years?—Those are the only two that I have received since I have been Medical Officer of the Board.

4525. That is in six years?—Yes.

4526. Have there been other experiments which come within the definition of this memorandum as being painful experiments?—There has been nothing which has been painful in itself as an experiment of inoculation; but, of course, the after results of some of the inoculations and feedings also are not without pain, or, at any rate, discomfort to the animal.

4527. But, apart from feeding or inoculation experiments, have there not in these six years been experiments on cats or dogs or monkeys other than those of feeding or injection?—There has not been one so far as I remember.

4528. I thought you spoke of a Report made to Parliament in the year 1901-2, and then you added that occasionally cats or dogs were made use of and rarely monkeys?—Yes, but the monkey has only been used for inoculation and feeding experiments; it has not been used for painful experiments.

4529. I suppose your knowledge only goes to this extent, that you have not received reports of any experiments other than feeding or inoculation experiments?—No, I have not. And I should receive a report of each experiment in detail, and should at once see if anything of improper sort had been done.

4530. No doubt you would if you got a report?—Unless it was excluded altogether and the bearing of the experiment was shut out altogether, I should certainly see something of it. I must necessarily see report of it. If it was undertaken in the elucidation of the problem that was given to the man to solve, it must be there.

4531. You get a report in any event in that case, whether the experiment was successful or unsuccessful?—Yes, it would be there; negative results as well as positive.

4532. I think you said that of the different operations that were desired by the Board, and made to your knowledge, something like 98 per cent. would be painless in the sense of being only feeding or injection experiments?—I think that would be the percentage. The exceptions are so rare that I think I am within the mark if I say 98 per cent. I have not taken the figure out.

4533. You gave us a list of the different officers employed, yourself and some others, and you said there were 13 inspectors?—Yes.

4534. What do they inspect?—They inspect the country generally as regards sanitary administration and vaccination administration; they hold inquiries as to loans for the establishment of hospitals, and especially they investigate epidemic outbreaks of infectious disease, and render advice and assistance to the local authorities generally in emergency. No doubt, as the result of what we now see in the papers as to the appearance of cerebro-spinal fever in this country, we shall have a great many suggestions made to us that an outbreak has occurred in this or that locality, and we shall send our medical inspectors down to ascertain the facts and to advise and assist the local authorities in dealing with the disease. A great deal of our work is in that direction.

4535. In your knowledge, have there been any cases in which a serum has proved to be valuable in a lower animal but has proved to be useless in the human body?—I think that probably, on considering it, one could find a serum or a protective agent which seemed to be efficient to a certain extent, say, in a guinea-pig, has shown, on further extension of it to the rabbit or some animal of that kind, that it was not so useful as anticipated, and that it was not worth while going any further with the preparation of it.

4536. With regard to the diphtheritic antitoxin, you said that it is now not made by the Board, because the use fell off so much?—The demand for it from the Board fell off, not the use of it. Its use has greatly increased. I am afraid I used a misleading expression. I meant the use so far as we are concerned had fallen off.

4537. I wanted to get that quite clear. The demand from the Board fell off, you meant to say?—The demand from the Board fell off.

4538. Is it within your knowledge that there has been a much greater use of it in recent years?—Yes, no doubt there has been a very large demand for it.

4539. And it is therefore obtained to a larger extent, though not from the Board?—Yes, it is a commercial product now, and can be bought of definite strength and under good guarantee, I think, from almost any chemist in the big towns.

4540. (*Dr. Gaskell.*) Burroughes and Wellcome supply it very largely?—Yes, they make it themselves, and Behring's import a considerable amount too, I believe.

4541. (*Mr. Ram.*) You say that when used by the Metropolitan Asylums Board, the result, which you attribute wholly, I think, to the use of this anti-diphtheria serum, is to bring down the percentage of deaths from 40 per cent in 1889 to something like 12 per cent. now?—I believe those are the figures as to cases treated in hospital.

4542. In the country generally there has not been a diminution of the total number of deaths from diphtheria?—The reduction of diphtheria mortality in the country would not be very great.

4543. I was suggesting that to you?—I suppose that is taking England and Wales. I believe there has been no very sensible diminution in recent years, certainly none parallel to the reduction of case mortality in hospital.

4544. Several suggestions were made to you as to
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what might be the cause of that. To what do you attribute it; have you any views upon that question?—In certain districts I know that diphtheria tends to occur for a few years and then disappears for a certain number of years; it is doing that, and has been doing that all over the country, and the balance of occurrence and disappearance leads to the result that over the country, as a whole, there is no very large diminution of the total mortality from the disease diphtheria. In some districts, of course, there is very great improvement, and improvement of long standing, whereas others are just now getting the diphtheria which has been deferred for them for a good many years, and they must suffer pretty badly from it until they take up the modern methods of prevention and cure, and then they will again get rid of it in their turn.

4545. Take the case of a scattered country district where there is an outbreak, chiefly among poor people and people inhabiting a poor class of house; do you think that the practitioners who attend them use the serum?—I doubt its general use curatively until recent years. But I believe a good many country practitioners use it now, and our Board is disposed, if a local authority seeks their assistance in the matter, to assent to expenditure on the purchase of diphtheria antitoxin by the local authority, and its administration by local practitioners at so much per administration; but as yet there has been no large demand on the Board for its use in that way, and, of course, what one would wish to see would be legislation making it uniformly available in the country for authorities who wish to use it.

4546. Take the case of an ordinary parish doctor. Supposing he desires to use this serum, how would he get paid for doing it; from whom would he recover the price of it?—He would have to ascertain whether the guardians would pay him for it. It is a very expensive thing to get it of considerable strength for use in considerable amount; therefore I have advocated for a good many years that the Board should make it available in the way I was saying, by Order; but I think it would be better to do it by legislation.

4547. Would you apply that general use of the serum both curatively and as a prophylactic?—Our business of course is chiefly preventive, and we should advocate it as a prophylactic, but I think if it is made available in that way, it should be available in both senses, both as a preventive and a curative agent.

4548. It is used by the Metropolitan Asylums Board both as a preventive and as a curative agent, is it not?—They use it curatively and they use it for their nurses, I suppose, as well; but the main use of it by the Asylums Board is undoubtedly for the cases of diphtheria (and there are many very severe ones), which are brought into their hospitals. I may say with regard to that that the diminution of the case mortality in the Metropolitan Asylums Board's hospitals must be a real fact, because the deaths in the hospitals are taken on the number of admissions, and the admissions for some years have not differed very largely as regards the severity of the disease. Therefore the reduction of case mortality of diphtheria is a fact; it is what is happening per hundred cases in hospital in each year.

4549. It is perfectly clear that it is those cases which are cured?—Yes.

4550. With regard to the diphtheria antitoxin, is its utility, in your opinion, now an established fact, and need there be any other experiments to prove it?—The value of the diphtheria antitoxin is a fact unquestionably, and I think that the statistics of the Metropolitan Asylums Board, which go a very long way, are corroborated from a great variety of sources. I think it is an accepted fact in the medical profession and in the preventive service, that the diphtheria antitoxin is certainly a highly valuable curative agent in diphtheria, and that probably, if we only had the opportunity of using it, people would find it highly useful as a preventive of diphtheria.

4551. Then is it necessary, in your opinion, to have further experiments on animals to prove that?—I do not think we want to do any more with diphtheria antitoxin. It may be possible, of course, to get it in a solid form, in a powder, or what not, so that it will keep better than it does now, but I do not think, administratively, we want anything better than we have got in the diphtheria antitoxin.

4552. Is there any other antitoxin besides the diph-

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Mr. W. H. Power, C.B., F.R.S. theria antitoxin of which you can speak in anything like the same terms?—I do not think there is any other—certainly not as a curative agent.

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4554. Would the experiments on animals necessary to arrive at such a result involve any pain other than that which might be caused by, say, illness consequent upon inoculation with the disease?—In testing a prophylactic on an animal, you have the primary injection in which you insert subcutaneously the preventive agent, and then at a later date you test the validity of the protection conferred by inoculating the "prepared" animal with virulent virus. The first inoculation, of course, causes practically no pain at all, nor does the second cause any if the prophylactic has been efficient; but if it has not, of course it causes the disease that you are trying to prevent, and that may be a painful disease. But in the case of a good many of these diseases which are induced in the rodents, the disease which is brought about by inoculation of the virulent virus is of short duration; the animals are very often killed if they do not die in a few hours after the inoculation, so that the pain is very transient in that case.

4555. If the disease is established in the animal you have arrived at that which you want to arrive at?—Yes, the attempted protection has failed.

4556. And the animal need not be kept alive any longer?—No.

4557. (*Dr. Gaskell.*) I just want to ask you one or two questions in order to make matters clear. These protective sera, you might say, are all based on the same sort of action, are they not; I mean, for instance, the production of the anti-bodies?—Yes.

4558. That applies also to Haffkine's plague bacillus as a remedy?—No doubt.

4559. So that what you desire is to get the anti bodies as clean as can be, and as powerful as can be, in order to get the specified strength, is not that so?—Yes, we want to get the anti-bodies and get them free, in supersession if possible, of the toxin.

4560. So that when you speak of a new remedy for plague which you are hoping will be better than Haffkine's, it is not a new remedy in the sense of being something different from Haffkine's, but only in the sense of being an improvement?—Quite so; it is essentially the same in general character.

4561. It is simply a further process of the same kind as Haffkine's process?—Yes, it is the same material, which is presumably present but in a less amount and perhaps potency in the Haffkine fluid.

4562. The anti-diphtheritic serum that was sent out is not so good after a time, is it?—No.

4563. It loses its power?—I am afraid it does.

4564. When that is sent out to local authorities or to medical men, is that fact carefully impressed upon them?—Do you mean by the trade?

4565. No, by you when you send it out?—When we sent it out we sent it out with a desire to know the result of its use, and, if I remember rightly, with an intimation that if it was not used within a certain time it had better be destroyed. But, as I was saying, that did not go on for a very long while, so that I hope there is none of that which we issued a good many years ago at present extant. We did not in those days attempt to standardise ours.

4566. I was wanting to know simply whether it was possible that certain failures which may have taken place in the use of this remedy were due to the medical men in question through ignorance having kept it too long?—It is quite possible. You are speaking of plague now?

4567. I am speaking of diphtheria now, because plague is another question?—The failure of the diphtheria antitoxin might have resulted through men having kept it too long. I should think possibly that when a man has spent a good many shillings in buying the material he does not put it down the sink very readily, but I have no facts to bear that out.

4568. You also said that the one antitoxin of which you could speak with some confidence was the diphtheritic?—Yes, certainly in regard to its curative property.

4569. Is that to a certain extent because your experience of the plague antitoxin is very limited?—Yes, we have nothing like the same experience in this country of the plague prophylactic that we have of the diphtheria prophylactic.

4570. Have you studied any of the statistics from India?—We have, but at home these are too few to base any conclusion upon. The prophylactic has been used here in a good number of instances, and in hardly any case has a person inoculated developed plague; but it has not been very certain that the persons have all been very freely exposed to infection. We have not had much plague in this country.

4571. I am not speaking of it in this country but in India. I have seen in Indian reports most remarkable statistics as to the enormous advantages of Haffkine's treatment?—I think the Plague Commission in India have thoroughly studied the Indian experience. I thought you were speaking of this country.

4572. I was trying to bring out whether the statistics would not show that Haffkine's antitoxin remedy was as beneficial for plague as the other one was for diphtheria, or very nearly so?—The diphtheria antitoxin is more largely used curatively; the plague serum is used as a preventive. The plague serum is very little used curatively.

4573. You have no knowledge of the statistics?—I have no personal knowledge of them. I only know what has appeared in the reports.

4574. (*Mr. Tomkinson.*) I think that practically all the questions I wished to ask you have been already put; but I should like to make one point quite clear. The number of operations upon living animals which came under your Department are practically confined to inoculation and skin operations, I understand?—Yes, scratches of the skin, pricks of the skin, and certain feeding experiments.

4575. And any real severity to which you allude (in which cases a report is expected to be made to you) results in the after effects of those experiments?—Yes, if there is pain or discomfort it will be in the after effects, but in the great majority of instances similar after effects would not be notably painful in the human being; they might cause some discomfort, but not more than that.

4576. The researches in respect of diseases in animals come under the Board of Agriculture entirely?—Yes.

4577. And the severe surgical operations are under the Home Office?—Quite so. Any severe operations that are undertaken in our investigations would certainly be reported in due course to the Home Office as well as to us. They would come under the supervision of their Inspector, and they would have to be done under their rules as to anæsthetics and killing the animal before it recovered from the anæsthetic.

4578. I think you said, though, that the chief of the experiments of research under your Department have proved to be curative rather than preventive of disease?—No; our object is to find a preventive rather than a curative.

4579. I know that is your object, but I thought you said the other way?—No; I think I may say that we are still looking for prophylactics, but, curiously enough, some prophylactics, as in the case of the diphtheria antitoxin, have proved better as curative agents.

4580. I rather gathered that the result of your experiments had been to mitigate the result of disease rather than to prevent it?—I think the outcome of some of the experiments on animals might be taken in that way, but that was not our object. Of course, we are partly seeing whether we could arrest a disease which has been actually established in the animal.

4581. Rather on the same lines as inoculation before vaccination was discovered, to mitigate the severity of the disease?—Yes; in a way. But sometimes the protective material is inserted into the animal at a date antecedent to the inoculation of it with virulent virus, and sometimes at a later date, after the virus has been inoculated.

4582. (*Dr. Wilson.*) You have said that none of the medical staff of the Local Government Board undertake any of these bacteriological investigations themselves now?—No, not now.

4583. So that in reporting on results, as it were, you have to depend in a very large measure on the conclusions which these experts arrive at from their experiments?—Not quite that. I have the details of the work before me, and I go into them very deeply. I have a detailed report from each man, and if I do not think his conclusions fit with his facts I want some further observations from him on the subject.

4584. May I ask who is the bacteriologist on whose investigations or advice the Local Government Board has chiefly relied?—The man who has done the most work for the Board in a series of years is Dr. Klein.

4585. May I ask whether, as he would be familiar with all the principal items of research during the last thirty years, the Local Government Board is likely to depute him to give evidence before the Commission or ask him to give evidence?—That seems to me to be entirely in the hands of the Commission. If the Commission intimate to the Local Government Board that they would like to hear one or more of the investigators who have been employed by the Board it would no doubt be the Board's wish that those men should come.

4586. All these newer methods, of course, are based upon Jenner's discovery of vaccination as a protection against small-pox?—No doubt they have followed from it.

4587. But do you see any strict analogy between the newer methods and Jenner's method?—I suppose there is analogy. Jenner's vaccine is supposed to manufacture locally at the point of the arm in which it is inserted a chemical substance which is subsequently disseminated through the tissues; whereas the preventive of modern days is obtained by manufacture of a chemical substance in artificial culture outside the animal body, for subsequent administration by injection into the body to be protected.

4588. In other words, the vaccine employed in vaccination must be, and always is, manufactured entirely in the living body, whether of the calf or of the human being, if it is arm-to-arm vaccination?—Yes.

4589. But with regard to all these other vaccines which are manufactured, the vaccines themselves are largely prepared, of course, from the bacilli of the particular diseases on culture media?—Do you mean the metabolic products of the life of the bacterium, or its intracellular substance?

4590. For instance, Haffkine's preparation?—Some of them consist of the bacillary body itself, the protoplasm of the micro-organism, some of its chemical products; and the two things may be separated in the sense that Dr. Sidney Martin has been investigating. Sometimes it seems to be the case that the best preventive may perhaps be derived from the body of the bacterium, and sometimes the chemical substance resulting from its metabolism seems to be preferable to the bacillary body.

4591. But the vaccine itself can, of course, be prepared outside the living body by cultivation on other media?—Yes, the protective agency is grown and multiplied outside the animal body instead of as in the case of vaccinia in the skin of the living body.

4592. That is a great difference?—That is a great difference.

4593. Then as regards results, when vaccine lymph is used all the symptoms follow a definite course, that is to say, if it is successful, you can predict that on a certain day the vesicle will be ripe and contain lymph for use?—Yes.

4594. And it is believed, of course, that although there may be a good many strains of vaccine lymph, all of them are more or less remotely derived from variola?—That is the general belief.

4595. And it is possible, is it not—it has been proved by experiment—by passing the vaccine from animal to animal through a certain number of cows to produce vaccinia from variolous lymph?—Yes, it produces appearances that we cannot tell from vaccinia, which afford the same protective influence as vaccinia.

4596. And which can be used as vaccine lymph?—Yes.

4597. So that really vaccination or vaccinia in the human being is successful, because it is a modified form of the disease of variola?—That is the contention.

4598. But do you see that any of these other vaccines or sera are at all analogous to that, either as regards the mode of production or as regards the pre-

cise symptoms which are developed afterwards?—Do you mean that you may get a chemical product equally as the result of life processes of two different micro-organisms in artificial culture media which shall have precisely the same effect in the animal body?

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4599. Yes?—It has been imagined, of course, that it could be done, but I do not think it has been fully proved.

4600. With regard to the anti-diphtheritic serum as a prophylactic it is very well known, of course, that medical men have so much faith in vaccination and re-vaccination that they always take care to re-vaccinate themselves if they have to deal with outbreaks of small-pox, so as to protect themselves by vaccination?—Yes.

4601. And you have also said that nurses occasionally in the metropolitan hospitals are protected by serum against contracting diphtheria, you believe?—Yes.

4602. Is it within your knowledge at all that medical men who are constantly seeing cases of diphtheria protect themselves against contracting the disease?—No, I think that they rely on that amount of immunity which seems to grow with increase of age, believing that they are less liable as they grow older, perhaps; or perhaps they think that they have got a protection from some previous diphtheria or *quasi* diphtheria, which satisfies them to go on without taking the serum. I do not think they take the serum.

4603. You have also admitted that in dealing with the case mortality of diphtheria there is a great liability to error in dealing with statistics?—Not so much in a given hospital which ordinarily receives the same class of cases; but in a country district, of course, you might get some wide differences in different series of years.

4604. The Board sanctions the free use of serum. I mean that it permits sanitary authorities to pay for the serum?—Yes, if you are speaking of the anti-diphtheria serum.

4605. Of course, you know more or less of the outbreak of diphtheria that has been prevalent in Hull of recent years?—Yes, I know of it generally.

4606. Two days ago I came across a review of an article by Dr. Hadwen, of Gloucester (whom I do not know personally, I may say, and with whom I have not communicated) to this effect. This is a quotation from his pamphlet. Dealing with the diphtheritic mortality—that is not the case mortality, but the mortality from 1891 to 1894, it then ranged between .05 and .08 per 1,000 persons living. "In 1895 (that was the year when the antitoxin was introduced) it rose to .11, and the following year to .17. There was a slight reduction during the next four years, which at no time went below the rate preceding the antitoxin period, and in 1901 the mortality stood at .15 per 1,000"—That is to say that there had been an increase in the mortality per 1,000 during the use of the antitoxin in Hull, is that the meaning of it?

4607. Partly so. But then this is the action taken by the Hull Corporation:—"Apparently to check this death toll, the Hull Corporation decided in 1901 to supply antitoxin free to the medical practitioners in the city." That would be with the consent of the Local Government Board?—Yes, it may have been; but a county borough can act in such case without reference to the Board.

4608. "The result was that in 1902 the diphtheria death-rate was more than doubled, viz., .34 per 1,000; and in 1903 it stood at .30 per 1,000. In spite of a slight lull to .24 per 1,000 in 1904, it rose again last year (1905) to .27." Now, of course I myself would not draw the inference that the use of the serum was the cause of the rise in mortality from the disease; but on the face of these statistics—you do not dispute them, I suppose?—No; but what occurs to me at once is that if in a big town you are rating the diphtheria mortality that occurs there on the total population, so long as your diphtheria is confined to one corner of the place you will have a low rate; but as it increases until it affects the whole you will get a higher and higher rate, and if its gradual extension towards affecting all quarters of the town is coincident with the use of the antitoxin (however useful the antitoxin might be in reducing case mortality), you would probably get a higher diphtheria death rate per 1,000 of the population of the complete town when the whole population is affected than when only a small portion of it was affected.

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4609. But you could not argue from these figures that the use of the anti-diphtheritic serum, even when distributed free to medical practitioners, has been followed by tangible results?—You cannot say that in that case there was the sequence which might possibly have been anticipated. But that is quite a different thing from saying that the antitoxin was not of use in that town in reducing case mortality. Moreover, antitoxin employed mainly as a curative agent could have but little effect in controlling spread of diphtheria in a town.

4610. But we know the result of using vaccine in an outbreak of small-pox?—It is used wholesale usually, and as a preventive.

4611. But with regard to this bacillus, has not the Local Government Board, according to your *précis*, been experimenting with regard to differentiation between the bacillus of diphtheria and Hoffmann's bacillus?—Yes, indeed.

4612. So that in diagnosing the disease, on the part of the bacteriologist there is great difficulty in coming to any reliable conclusion as to particular cases?—Quite so. That was one of the reasons why the Local Government Board instructed one of their investigators, Dr. Mervyn Gordon, to differentiate (if it were possible to differentiate with certitude) the diphtheria bacillus from other varieties of diphtheroid bacilli by culture media alone, because otherwise the confusion would continue, unless the animal test were universally resorted to. With no great success, perhaps, but with a certain measure of success, a considerable amount of work has been done in that way. But it is the negative aspects of the culture test which are the difficulty in the case of diphtheria, as no doubt you have found.

4613. Is it your experience, or within your knowledge, or do you think it probable that a great many throat cases are notified as cases of diphtheria which would not have been regarded as diphtheria cases in former days?—If their clinical symptoms alone had been regarded, I think so. I think the bacteriological test probably increases the number of cases notified.

4614. So that that also would tend to vitiate the case mortality rates in the hospitals of the Metropolitan Asylums Board?—That would not affect the case mortality in hospitals, but it would in the country generally.

4615. And in hospitals?—In hospitals the cases are all practically admitted as diphtheria cases.

4616. But in the Metropolitan hospitals is it not a fact that a good many cases have been returned and sent home again, or reported as not being cases of diphtheria at all?—Yes, then they would not be taken account of in the statistics.

4617. Does not that show that the hospitals are very freely used even for slight cases, and cases regarded as doubtful—not serious diphtheria cases?—Yes, but we were speaking of case mortality before and after the use of the anti-diphtheria serum among hospital cases. If under both circumstances, before and after its use, the doubtful cases were weeded out, and only unmistakable diphtheria treated in the hospitals, then less case mortality under the use of the antitoxin than before its adoption would certainly tend to show that the antitoxin was of value curatively.

4618. But supposing that in the old pre-antitoxin days only severe cases were sent, or the more severe cases, to the hospitals, and that in recent times people have become so familiarised with the advantages and uses of hospitals that even suspicious or slight cases are sent, would not that of itself tend to vitiate the statistics greatly?—If a different class of cases was admitted to the hospitals at one period than at another, of course it would. If mild cases were admitted at one period, and none but unmistakably severe cases were admitted in the other, that would affect the fatality statistics, of course.

4619. As regards those milk outbreaks, it is not believed that the cow really does suffer from diphtheria, is it?—It has been experimentally shown that by inoculating diphtheria material into the subcutaneous tissue of the shoulder of the cow, the cow will suffer from a malady which is characterised by roughness of coat, loss of hair, and perhaps eruption on the teats, and that that cow will excrete in its milk the diphtheria bacillus. How far that accounts for the milk diphtheria epidemics is another matter.

4620. Of course, in diphtheria it is not the bacillus

that does the mischief; it is the toxin derived?—The toxin produced locally, and subsequently disseminated through the tissues.

4621. Is it not probable that these were merely septic outbreaks? Was there not some difference of opinion at the time amongst bacteriologists?—If you mean that there may be some micro-organisms besides the diphtheria bacillus that will secrete a ferment and which can act on cows in a similar way, it is possible thus to account for outbreaks of throat illness referred to milk.

4622. That is to say that you have to depend entirely on the bacteriologists for the translation of these results?—No doubt they help us to interpret what occurs under our eyes very largely.

4623. During all these years have any of the bacteriologists employed by the Local Government Board discovered any bacillus or micro-organism which has been accepted by bacteriologists as the cause of any particular disease?—I should have to think how far our men have contributed to the knowledge which has been acquired throughout the world. You see each disease is coming under review now, and is tested biologically in all sorts of ways, and whether we have been absolutely first in establishing anything I could not say. I do not think we have tried to claim it, even if we have done it.

4624. Then the only serum, or vaccine rather, which has been issued by the Board on its own responsibility, as it were, after testing it and so on is this plague vaccine?—No, we have issued the diphtheria antitoxin and also Haffkine's fluid.

4625. But that was not discovered by Dr. Klein?—We did not discover it nor did we discover the Haffkine material. In fact what we made and issued we made in collaboration with Haffkine himself, who happened to be in this country at the time. Dr. Klein and he made and tested it together.

4626. Then although the Local Government Board grants facilities in urging or encouraging sanitary authorities to use, say, the anti-diphtheritic serum, they have not issued any specific instructions as to how it should be used?—No, we have not done that.

4627. And with regard to the diagnosing of diseases, the only disease concerning which specific instructions have been issued is plague, is it not?—No, we undertook at one time to assist in the diagnosis and identification of first cases of diphtheria in a locality. Of course we have done that with cholera also, and we have done the same thing with plague. And we shall probably be doing—we have done already to a limited extent—the same thing with cerebro-spinal fever; and in influenza we have attempted to do something, but I am afraid it did not result in very much.

4628. Dr. Klein, I suppose, would be the bacteriologist employed to decide in these questions?—Not necessarily; it might be Dr. Klein; it might be somebody else. For instance in the case of the meningococcus, it is Dr. Mervyn Gordon who has worked at streptococci and staphylococci more than any of our experts.

4629. Now supposing a severe outbreak, as we have had occasionally of influenza passed over this country, would it not be extremely difficult to distinguish between a case of pneumonic plague and pneumonic influenza?—I do not know. I suppose in pneumonic influenza a suspicion might arise that it was plague if there had been an opportunity for the person to be exposed to the infection of plague. No doubt we have had sent to us at the Local Government Board the material from cases of pneumonia which have arisen in circumstances in which there might possibly have been exposure of the individual to the infection of plague, and we have tested such material to see whether plague was concerned after all in the current disease. In fact we get a great many suspected cases which turn out to be nothing exotic; they greatly outnumber the positive cases.

4630. In your *précis* I see you quote from Sir John Simon. Do you think that the hopes which were so largely entertained by him have been in great measure realised?—I think they are in process of fulfilment. I do not think that Sir John expected that knowledge would come very fast; in fact I have often heard him speaking the other way—he thought that a great many of our enquiries must necessarily proceed by slow degrees.

4631. So that all the practical work and really,

valuable work of the Board is carried out on the old lines, that is to say by practical sanitation and disinfection and isolation?—Quite so; much of it. But the practical work of scientific sort undertaken by us on the question of disinfection of ships-holds with cargoes *in situ* has been of extreme value. That question administratively has been a very difficult subject indeed, owing to the very strong notion among Continental nations that the rat was so dangerous, *qua* transmission of plague, that all ships that came from a country in which there had been any plague at all within certain months, and therefore of course from every port in India, should have their rats destroyed before they discharged their cargo. That meant if it were to be adopted generally, not only that “healthy” vessels would be detained while that was being done, but that a great deal of cargo might be spoilt if sulphurous acid was used. Accordingly we had to go to work to find out whether any other gaseous disinfectant, for instance carbonic oxide, could be used, or under what conditions sulphurous acid could be used with the least disturbance of trade and least detriment to the cargo; and Dr. Haldane’s and Dr. Wade’s series of experiments on this matter of the destruction of rats and disinfection on board vessels retaining their cargoes *in situ* have proved very valuable.

4632. (*Chairman.*) There is just one question I should like to put to you. You spoke of a period when in the London district by the use as you suggest of this antitoxin the number of deaths from diphtheria was reduced from 40 per cent. to 12 per cent., I think?—I think that it was of the proportion in the Metropolitan Asylums Board’s hospitals that I was speaking.

4633. At that time were the Metropolitan Asylums Board’s hospitals all using this antitoxin?—There was a period in which they did not use it at all, and then quite suddenly it came into use, and the contrast was afterwards very marked indeed.

4634. Did that drop from 40 per cent. down to 12 per cent. coincide with the time when the Metropolitan Asylums Board’s hospitals all took to using the antitoxin?—It was coincident with the modification of procedure. I do not think the adoption of the diph-

theria antitoxin was universal in one year in all the hospitals. I think it was gradually adopted until in the end it was used in all the hospitals. You would thus get that sequence.

4635. My question was directed rather to another point. There were a number of consecutive years during which the mortality fell in the Metropolitan Asylums Board’s district from 30 or 40 per cent. down to 12 per cent.?—I believe that was the figure.

4636. Whilst the antitoxin was being used?—Yes.

4637. Now I want to ask you whether you can give us any idea to what extent during that period the antitoxin was used in the rural districts, the districts outside the Metropolitan Asylums Board’s district?—I do not think that in those days it had come to be adopted at all generally outside London. I think it was the experience of the Metropolitan Asylums Board that encouraged later on its use so much throughout the country by sanitary authorities.

4638. I understood you to say that while the rate was dropping from 40 per cent. or whatever the figure was to 12 per cent. in the Metropolitan Asylums Board’s district, there was no great change in the rest of the country in the case mortality?—I do not think there was. I should have to look it up in order to speak precisely.

4639. I will take your answer to that effect—you may have to correct it. Assuming that to be so I want to see how that was connected with the use of the antitoxin. I understood you to say that the antitoxin was being adopted, at any rate, much more slowly in the country?—I do not think it was adopted at one and the same moment in every hospital of the Metropolitan Asylums Board; and just in the same way its use gradually spread in the country generally. But it was the experience of the Metropolitan Asylums Board as to its curative value that brought about its more general adoption in the country; so that there would be that sequence; the experience of the Asylums Board was obtained and made known before the country began to benefit.

4640. It began later in the country than in London?—Yes; there can be no doubt as to that, I think.

Mr. W. H.
Power,
C.B., F.R.S.

20 Feb. 1907.

ELEVENTH DAY.

Tuesday, 26th February 1907.

PRESENT :

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
Sir W. S. CHURCH, Bart., K.C.B., M.D.
Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.
Mr. W. H. GASKELL, M.D., F.R.S.
Mr. G. WILSON, LL.D., M.D.
Mr. J. TOMKINSON, M.P.
Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. A. R. CUSHNY, M.A., M.D., called in; and Examined.

4640A. (*Chairman.*) You are Professor of Pharmacology and Materia Medica in University College, London?—Yes.

4641. And you appear here at the request of Professor Starling’s Committee, as we call it?—Yes.

4642. Do you yourself hold a licence?—Yes.

4643. And perform operations on animals?—Yes.

4644. Are your operations directed generally to general physiological research, or to testing for particular antitoxins for particular diseases, or experiments of that kind?—My work is generally directed to elucidating the action of drugs, particularly new drugs—not toxins so much as chemical drugs and plant drugs, and that sort of thing, not the actual animal toxins.

4645. In fact your work generally is in the direction of the work described in the paper you have sent in to us?—Yes.

4646. On the first page of your paper I see you give a general description of the object of your evidence?—Yes.

4647. Would you tell us what that is?—I desire to explain the influence which the experimental method in pharmacology has exercised on therapeutics in the course of the last half century, during which the study of pharmacology has been carried on by the examination of animals. Perhaps I ought to explain exactly what I mean by pharmacology. Pharmacology means the examination of the action of drugs on animals or from clinical experience. It includes both, but the object is to explain the effects which one sees in

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patients through methods of direct observation in animals, and the two are co-ordinated so far as possible. My object is to explain the way in which drugs act in patients when we cannot see how they act, by examination of their effects on animals either in health or disease, particularly in health.

4648. You mean what are the methods of the operation of drugs on animals, and on life?—Yes, exactly.

4649. It has been, I think you say, a matter of clinical observation so far as it can be proved by clinical observation for many years?—Since all time practically, since the earliest historical records.

4650. And I see you refer to emetics and purgatives as being instances of the more obvious and better understood subjects?—Yes. Animal experimentation has added very little to the knowledge of the effects of purgatives, which, of course, are perfectly obvious, and also comparatively little to the action of emetics except in adding one or two—but of course it was perfectly obvious; and you find purgatives and emetics among the very earliest drugs, particularly purgatives.

4651. But that is not the case with a great many drugs. A great many advances have been made on that common knowledge of late years?—Yes, drugs, for example, which act upon the brain or act upon the heart or act upon the liver or kidney which have not such obvious effects have been really worked out in detail by experiments on animals. You cannot see the changes in the heart, for example, as you can see the effects of increased activity of the bowel or one of the other more obvious organs.

4652. You propose, I think, to give us an example the discovery, or the more precise discovery of the action of digitalis?—Digitalis was introduced into medicine by Withering about 1785, who found that it was a remedy for dropsy; and it was used in dropsy without understanding in the least how it acted for a good many years. In fact, the fact that it acted upon the heart mainly was brought out first by Traube and Brunton about 1860 to 1870.

4653. Digitalis is, as its name denotes, taken from the foxglove, is it not?—Yes. I wish to point out the influence which this discovery of Brunton and Traube had upon the use of digitalis, and for that I take the statement of a professor of materia medica who wrote a book, *Handbuch der Speciellen Arzneimittellehre*, about 1860, namely, Clarus, who says that digitalis slows and weakens the heart, and draws the conclusion that it is useful in aneurism and other similar vascular enlargements, and in acute fevers. This was the result of about 75 years of clinical observation. Within a few years Traube and Brunton showed by experimental methods that one effect of digitalis is to raise the blood pressure to a marked extent, which is exactly the worst treatment possible in aneurism.

4654. That is to say to raise it instead of lower it?—Yes. No one would dream of using digitalis in such a condition at the present time. And perhaps I ought to state that Traube and Brunton showed a new action of digitalis which had not been suspected, an action upon the vessels, which increased the blood pressure. Acute fevers, and especially pneumonia, were still treated with digitalis, however, until the exact seat of action of digitalis was determined by experiments performed on animals by Brunton, Schmiedeberg, and others to be the heart muscle.

4655. Stopping there a moment, would you explain to us a little more what was the nature of those experiments. On what animals were they employed, and to what extent?—The animals were partly frogs and partly mammals, and, so far as I remember it, the animal was anæsthetised, and the carotid artery in the neck exposed and attached by a tube to a mercury manometer, so that the pressure could be observed in the artery; and generally the movements of the mercury were recorded in the manometer, and the digitalis was then injected into a vein. The effect of injection was to increase the pressure in the artery, and therefore to raise the mercury; this rise was recorded, and lasted for some time, and was shown to be the result of the drug.

4656. Could that experiment be effectually made on a frog?—No.

4657. What were the other animals used?—Rabbits and dogs. I think Traube used dogs, so far as I remember.

4658. And were those operations performed in Eng-

land or in Germany?—Traube worked in Germany. Brunton worked partly in Germany and, I think, partly in this country.

4659. (Sir William Church.) In Edinburgh?—Yes, in Edinburgh.

4659A. (Chairman.) At any rate in Great Britain?—Yes.

4660. Were those experiments before the present Vivisection Act?—Yes, they would be before 1875.

4660A. And do you know at all to what extent anæsthetics were used?—I am quite sure Brunton would use anæsthetics, and I think the others would have used them, too, for this reason, that with the manometer you could not get any satisfactory record without anæsthetics; the animal's movements would cause too much irregular movement of the mercury, and one could not make out the changes.

4661. And would the animal recover from an operation of that sort, or would it be killed by the operation?—It could be recovered, but it would not be recovered; it would not be allowed to recover.

4662. (Mr. Ram.) Before the Act?—Yes, there would be no object in recovering it, I think.

4663. (Chairman.) You are not speaking of operations that you were present at, but your belief is that anæsthetics would have been used for the sake of the success of the experiment itself?—All I can say is I should certainly use anæsthetics in such a case, even if it were not rendered necessary by law, because it would be quite impossible to carry them out satisfactorily without anæsthetics.

4664. Was that set of experiments complete before the Act of 1875, so that there was no occasion to experiment further for the purpose of ascertaining what the action of digitalis was?—No, they were not complete. Brunton and Traube and Schmiedeberg showed that the heart was affected, but at that time the knowledge of the physiology of the heart was so imperfect that they could not go any further. It was not until the physiology of the heart was developed that the exact way in which the heart was affected could be worked out, and, as a matter of fact, I have done a good deal of work in that subject of digitalis myself ten years ago; and I find new problems coming up at the present time. I have been appealed to by clinicians to explain some further effects that they have got quite recently.

4665. We will deal with your more modern experiments presently. Having explained what the operations were by Brunton, Schmiedeberg and others about 1860 and between then and 1875, you say they discovered that the action of digitalis was upon the heart muscle?—Yes. This gave definition to the use of digitalis which was absent before; it is useful in pneumonia only when the heart is affected. The suggestion arose from their experiments that it was useful in pneumonia only where the heart is affected, while in other forms it may do more harm than good. This example of the influence of experimental research in therapeutics might be repeated in regard to dozens of drugs, were it necessary. It may give a better impression than a general statement of how the discovery of the exact action of a drug reacts on its therapeutic use by lending it greater precision. Instead of using such a drug as digitalis as a routine treatment in all cases of pneumonia, it defines the conditions under which it is likely to be of benefit, and excludes others in which it is not necessary. Doubtless, it may be objected that clinical observation alone would have led to the same result in time, and that, as a matter of fact, experimental results can only suggest a clinical examination of the truth of a theory based on them.

4666. When you say that it would have led to the same result in time, I understand that the operation of digitalis had been under clinical observation for 75 years before 1860?—Yes.

4667. Was there any immediate prospect at that time in 1860 of their discovering what Brunton and Schmiedeberg discovered, by clinical observation?—I do not think so. I think the knowledge of the action of digitalis prevailing, say, in 1785, was about as complete as the knowledge that they had before Brunton and Traube. Very little had been added to the knowledge, and much that had been added was false.

4668. But we are often told that it is a toss up who

is the particular discoverer of any new discovery; that there are several just on the point of discovery. Was anybody on the point of discovering by clinical observation what these other gentlemen discovered by experiment?—No, I think not. Traube had clinical interests; he worked as a clinician, and his work in physiology was largely suggested by his clinical work; and he went to animals to work out how digitalis acted, in fact, because he evidently failed to do so in his patients. Brunton's interests, of course, were always clinical, and apparently he went to animals in order to explain the problems he could not solve in patients.

4669. He was not satisfied of the accuracy of the theory about the action of digitalis before him?—Yes, that is what I understand. The danger of trusting exclusively to clinical examination for therapeutic advance may be exemplified by the same drug. It is undoubtedly of value in certain forms of pneumonia, and is useless or deleterious in others. If it were adopted in all cases the mortality statistics would probably be scarcely different from those of cases not treated with it, and the result would be the rejection of the drug. The heart action discovered by experimental methods, however, directs the clinician's attention to this phase of pneumonia in relation to digitalis, and he finds that the statistics of cases of pneumonia in which the heart is involved are improved by digitalis.

4670. These experiments of Brunton and Schmiedeberg and others were not experiments for other purposes in general physiology, but they were experiments directed specially to the action of digitalis?—Yes.

4671. You have told us what they discovered, and you say that further experiments have given more precise knowledge still?—Yes.

4672. And you think they are leading to still further precise knowledge?—Yes, they are leading to a more precise knowledge of the exact point of the action of digitalis on the heart.

4673. We cannot foresee what other witnesses' opinions may be, and there may be some who contest that. Perhaps you would explain, if you can explain, what the precise further discoveries are?—It is a little difficult to put it into words.

4674. I do not mean exactly, but the direction of them. You have told us that they discovered that digitalis acted on the heart muscle?—Yes.

4675. Does that remain true?—That remains true, but it is further defined. The idea arose about that time, partly from Schmiedeberg's work, that the contraction of the heart was prolonged under digitalis, whereas this is not true in mammals, at any rate; the contraction of the heart is not prolonged, but is strengthened only, it is not actually longer than before. And then the further point came out that digitalis acted in two ways. It acted first on the heart muscle and then indirectly through the controlling nerve of the heart, the pneumo-gastric. And those two points are of considerable importance, because it is found, for example, in patients, that sometimes the heart is not slowed under digitalis at all owing to this controlling nerve not acting, while in ordinary persons the heart is distinctly slowed through the control nerve being active. Now, one point is that this slowing of the heart is so common that physicians are apt to regard it as an indication of the action of digitalis; and when the slowing is absent, they think the digitalis is not acting and give more digitalis, give larger doses. But in certain cases this slowing is absent, or is quite slight in amount, and yet the full action on the heart muscle is taking place; so that there may arise a danger from larger doses than are actually being given. Later work showed that one must not depend too much upon the slowing of the heart which had been supposed to be quite a sure sign of the action of digitalis.

4676. Would you say that that further discovery, that digitalis acted through the nerve in the way you have described, was a thing not merely to satisfy what you may call a physiological curiosity on the part of the operator, but was something practically useful to a doctor in practice?—Yes, I think so, simply in showing the doctor that this secondary action upon the muscle direct might be present when the slowing was absent. In certain fevers, for example, one finds that there is no slowing, or very slight slowing, and yet the direct muscular action is going on.

4677. It would assist him in knowing what the exact condition of his patient was and what remedies he ought to apply?—Yes, indicating that he ought to be careful in applying more digitalis.

4678. (*Sir William Church.*) Was the action of digitalis on the walls of the vessels known before?—It was known; at least, it was suspected from the time of Traube and Brunton.

4679. But not before?—No.

4680. You might have mentioned that perhaps as another point which was discovered of the action of digitalis?—I intended to bring that out when I said that their essential discovery was increase of blood pressure through a new agency, which had been missed entirely before, namely, the action on the vessels as well as on the heart. All the work since Traube and Brunton has been to show that digitalis is a much more complicated problem than was supposed before their time, that it affects a much larger number of systems.

4681. (*Chairman.*) Were these other experiments which led to the discovery of the action on the vessels of the heart experiments on animals?—Yes.

4682. And are experiments directed to increasing that knowledge now going on from time to time in regard to digitalis?—Yes, there is a constant series of new questions coming up. As one question seems to be decided, it is found that it only suggests a further one. The knowledge of digitalis which has been obtained in the last quarter of a century is much greater than it had been ever since its discovery until that time. Really the first discovery regarding digitalis since Withering's time was this of Traube and Brunton of the vascular effect; then Schmiedeberg's of the heart; and since then a large number of workers have been elucidating its action further in different directions.

4683. Have those experiments been experiments of the same description so far as regards the operation as those you described before?—Of recent years much more work has been done on mammals under digitalis by exposing the heart. That is the essential change that has come. The heart is exposed instead of taking the indirect method of examining through the vessels—the heart itself has been exposed, and its movements have been observed, and have been recorded.

4684. That, I presume, is an operation that is done under anaesthetics?—Yes.

4685. And is the animal killed before it recovers?—It cannot recover from an operation like that.

4686. Then I presume it is killed in the usual way when the operation is over?—It is killed at the end of the experiment, certainly. In those experiments where the heart is exposed, of course the respiration has to be carried on artificially, and if the animal does not die of a large dose of digitalis, as is ordinarily the case, it dies at once when the respiration apparatus is stopped.

4687. Then those are cases in which the ordinary rule would apply, that anaesthetics would be administered, and the animal would not be allowed to recover consciousness?—Yes.

4688. And they would not require any certificate?—No.

4689. You have given us in digitalis the case of the correction of an error about what was the effect of a drug in use. Now I think you are going to tell us of examples in which experiments have guided you in knowing whether a particular drug or medicine should be accepted for use at all?—Yes. Another result of experimental inquiry is that the physician demands more certain evidence of curative action before accepting a drug. For many years lead had been used to arrest hæmorrhage from the lungs, the lead being given by the stomach generally in the form of pills. Its efficacy was unquestioned, apparently, by physicians until experimental inquiry elicited the fact that lead is only absorbed in small quantities, and very slowly. This, of course, does not imply that it is useless in bleeding of the lungs, but it removed the basis of the underlying theory on which it had been used, and aroused the clinicians to more careful analysis of the cases in which they had previously used it, the result being that it was found that not the lead but the opium with which it had been administered was the efficient principle in arresting the hæmorrhage. So that experimental inquiry in this instance, as in many others, is

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Mr. A. R. Cushman, M.A., M.D. leading to the elimination of treatment which was altogether devoid of value, and not unattended with deleterious consequences.

26 Feb. 1907. 4690. Lead if taken in certain quantities is, I suppose, a poison?—Yes.

4691. And if it does not do good it may do harm?—Yes.

4692. That discovery, I presume, caused physicians to abandon the use of lead for this purpose?—Yes. It is a recent discovery, and one still finds that it has not permeated into some clinical circles, where lead is still used to a certain extent; but it is dying out.

4693. You say that it was discovered by experimental inquiry. What do you mean precisely by that?—Lead was given to animals, and it was looked for in the urine, and not found there; the whole of the lead, or practically the whole of it, could be recovered from the bowel; there was no lead practically excreted, and there was no lead found in the blood when it was examined after lead had been given in animals.

4694. (Colonel Lockwood.) All the lead which had been given, not one particular dose; but if you dosed a dog say, for a week, could you recover it all?—It is not a question of a week. It is the acute action that one wished to ascertain.

4695. (Chairman.) It is given to stop hæmorrhage?—Yes, it is to stop hæmorrhage, which has to be done, of course, at once.

4696. Did that experimental inquiry that you speak of, of examining the blood involve any operation?—Blood would have to be drawn; blood would have to be taken from an artery.

4697. To that extent it would be a painful operation, but not beyond that?—No.

4698. And not necessarily a fatal one?—No; the animal might live quite well afterwards.

4699. And would the lead that was given to a dog be in quantities that would affect its life?—No, it was not given in large quantities. It was given in larger quantities than would be given to a man, and also in small quantities, what would be in ordinary doses to a man, in order to compare whether the effect was due to the dose.

4700. Were those operations that required a licence, taking blood from an artery for the purpose of testing it for lead?—It would have to be done under licence, or under licence and a certificate in this country.

4701. If done without anæsthetics?—Yes.

4702. As it would be, I suppose?—No, it would be done under anæsthetics.

4703. But you suggest, I understand, that it was a discovery which was made by experiments of this kind?—Yes.

4704. It had not been made by clinical observation?—No.

4705. Was it in process of being made by clinical observation?—There was no sign of it at all.

4706. It seems obvious the discovery was a useful one, that is to say, you ceased to give lead, which is a deleterious substance?—Yes, in merely eliminating lead.

4707. What was substituted for it—opium?—Opium is given alone largely—opium or morphin.

4708. Those you speak of as being examples, digitalis, and lead in hæmorrhage?—Yes.

4709. Could you, if need be, give other examples of other drugs?—Yes.

4710. Are these the two single instances that you know where such a thing has happened, or are they merely examples of the kind of discovery that is going on?—They are merely examples. I could give large numbers of other instances in which practically the same thing was done, on either the introduction of new drugs or the introduction of new points in regard to drugs or the elimination of useless drugs.

4711. Generally speaking, I presume useless drugs are hurtful drugs to a certain extent?—They may be hurtful; they may be useless, or actually harmful. Numberless examples of this increased accuracy in the therapeutic use of drugs might be cited as the results of the minute examination of their effects in animals. Many old drugs have been discarded as the result of these inquiries, while others have proved to have properties which were previously unsuspected. The physi-

cian now has a much clearer view of what his remedies can do, and prescribes with a definite purpose, while formerly he could apply them only on the general knowledge that they were followed by favourable symptoms in some cases resembling more or less closely the one in hand. Until the exact method of action of a drug is known, it cannot be realised what condition it is likely to benefit. The tendency is to treat diseases as a whole, rather than to ascertain the exact phase of the disease which is to be attacked. I would suggest that the usefulness of pharmacology lies in breaking up disease into symptoms, each one of which can be treated with drugs, when those symptoms are present, instead of treating the whole disease with drugs. The experimental method has so defined the action of a drug that we can apply it much more accurately than before.

4712. You now go on to speak, I think, of the introduction of new drugs as one of the results of experimental methods?—An immense expansion of the resources of the therapist has occurred as the direct result of experimental inquiry. I can give a number of groups of drugs which have been discovered in this way. The first group I will speak of is the soporifics, or sleep-bringing drugs. The first of these to be discovered was chloral. Up to 1868 the drugs used to produce sleep and allay restlessness and nervous excitement were opium and its derivatives, hyoscyamus and Indian hemp, and potassium bromid, but of these opium and bromid alone were in common use. In 1868, Liebreich, of Berlin, took up a problem of purely scientific interest, namely, whether "a substance is broken up into its constituent parts before it is oxidised." And for this purpose he injected trichloroacetic acid and chloral into frogs and rabbits. These two bodies had never been used in therapeutics or in experiments before, and nothing was known of their effects on the organism. Liebreich was so interested in the effects of chloral in causing sleep in animals, that he abandoned the primary object of his research and devoted his attention to a long series of experiments on its soporific action in animals. He states, at the end of an account of these, that "the foregoing experiments on animals give us such a precise knowledge of the method of action of chloral, that it did not seem rash to commence with its use in man." He accordingly tried it in a number of cases in order to combat sleeplessness, with success, and since the publication of his results, chloral has assumed its present position as an invaluable hypnotic. It has been included in all the pharmacopœias.

4713. When you speak of a long series of experiments on its soporific action on animals, what was the nature of those; giving them doses of chloral and seeing whether it sent them to sleep?—He injected it intravenously at first into a vein in dogs and rabbits, I think. Then he tried whether he could get the same effect by injecting it hypodermically, I think, and finally he gave it by the mouth.

4714. As used now as a soporific, is it used in any way except by the mouth?—Only by the mouth.

4715. And these were experiments on animals to discover which was the most effective way or the most harmless way?—The most harmless and effective way. Chloral was used in man sometimes by a vein at first also. It was not realised that the full effect could be got by the mouth.

4716. Would there be any danger in the case of taking chloral by the mouth in making those experiments on man instead of on animals?—Chloral is a poison as well as a drug. It depends entirely upon what dose one commenced with. If they had begun with a large dose there would have been very great danger indeed.

4717. But that you would not do, I suppose, if you were experimenting on man; you would begin with a small dose?—Yes, I should begin with a small dose if experimenting on man.

4718. Are small doses found effective?—A dose of two or three grains probably would have no effect at all. If one advanced the dose to 8, 10, or 12 grains, one might get some effect in man, and with 15 grains there would be a pretty obvious effect of sleep.

4719. I do not quite see why it is necessary to introduce the animal; I am not saying whether it is justifiable or not, but why is it necessary in this particular case? Supposing you had begun experimenting with a small dose on a patient, for example, who

wished to sleep. I suppose you would wish to be quite sure that it would send him to sleep in the first instance?—But Liebreich had not the remotest conception that it would cause any sleep at all, it was a pure chance. He did not give it to his animals to cause sleep; he chose chloral because it was a substance of which he could detect the constituents in the blood. It was a purely scientific inquiry he was engaged on; no one had any conception at the time that such a drug would cause sleep.

4720. I was referring rather to the experiments you speak of when you state that he devoted his attention to a long series of experiments on its soporific action in animals?—It was not so much its soporific action—perhaps I put it badly—it was to find whether there were any other effects besides the soporific; for example, whether it acted strongly upon the heart, which would be a bad thing; whether it was eliminated rapidly by the kidney, and whether other organs were deleteriously affected, or whether it was purely a sleep drug.

4721. (*Colonel Lockwood.*) What is it made of? What are the component parts of it?—It is made from alcohol and chlorin. It is not very far away from chloroform.

4722. (*Chairman.*) You would not have thought it safe, I understand, to experiment on mankind; there might be something harmful in it?—Yes. Liebreich had to find out not only its sleep-bringing property, but also whether it had any poisonous properties in addition.

4723. Were these experiments experiments which involved death to the animal?—Yes, I think most of them were manometer experiments. As a matter of fact, the animal would mostly die of chloral afterwards, with the large doses he used, anyhow.

4724. Or would an injection into the veins kill it?—Not necessarily.

4725. Were these experiments after the Act of 1875?—No, in 1868 and 1869.

4726. And they were in Germany?—Yes, they were in Germany.

4727. At any rate the result of them was to introduce chloral as being a safe and useful soporific, I understand?—Yes.

4728. And has it been adopted generally in medicine?—Very largely.

4729. And, as I understand you to say, after, and in consequence of these experiments of Liebreich?—Yes.

4730. Then what are the other soporifics?—The second example I take is sulphonal. While Baumann and Kast were examining the changes in organic sulphur compounds in the body (a research in physiological chemistry of purely scientific interest) their attention was attracted to the action of a group of bodies, the disulphones, on a dog, which went asleep and only regained its normal condition a number of hours afterwards. Kast examined this phenomenon in a long series of experiments on different animals, and then took occasion to test the effect on man. The successful results induced him to introduce into therapeutics the very valuable hypnotic sulphonal, which he found the best of the disulphones.

4731. I understand both these discoveries of the use of chloral as a soporific and of sulphonal arose incidentally out of experiments which had an object of general physiology?—Yes, they were casual notes, casual observations, so far as therapeutics are concerned.

4732. And the particular experiments out of which they arose in the first instance were not experiments with the view to discovering a remedy for any particular disease?—No, they had no reference to therapeutics.

4733. Then other soporifics that you refer to?—Later, comparisons of the effects on animals led Kast to prefer the analogous compounds trional and tetral as somewhat better, and they have also been used in therapeutics. In 1881, Cervello examined the effects of a number of bodies on animals, among them paraldehyde.

4734. Were those in Italy?—I think he began them in Italy and completed them in Strasburg, under Schmiedeberg. They are published in Schmiedeberg's Archive. He expected this compound to act like aldehyd, and was much surprised to find it cause

sleep. He advised its use in medicine, and it is now included in most pharmacopæias, including the British Pharmacopæia.

4735. How does aldehyd act?—Aldehyd is an irritant body that cannot be taken on account of its causing irritation of the stomach and irritation of the throat. Paraldehyd is very closely related to it chemically.

4736. But it has not its deleterious effects?—They are almost entirely absent; in fact, they are absent in most cases.

4737. And that experiment also was the discovery of a remedy arising out of general inquiry?—Yes. Then Crum-Brown and Fraser were the first to point out as the result of animal experiments that bodies resembling each other in chemical composition often induce somewhat similar effects in the animal body, and the discovery of these sleep-compelling compounds suggested the view that all bodies of a chemical composition similar to them would act as useful hypnotics. As a matter of fact, this is only partially true. Each body has to be tested on animals to find whether it has hypnotic properties which can be utilised in practice, and comparatively few have stood the test. Liebreich's introduction of chloral has, however, proved to be the first of a very considerable group, all introduced into medicine by the same way of animal experimentation. These are amylene hydrate, urethane, hedonal, neuronal, chlorotone, isopræl, chloralamid, chloralose, bromoform, and last of all, perhaps the best of the series, veronal and propional. No soporific has been introduced in the last forty years, except by means of animal experiment.

4738. Do you consider that these could any of them have been safely introduced as an ordinary remedy for man without experiment first on animals; is that your view?—They could not have been introduced without experiment on animals. Perhaps I might give you an example of that in the dose. For example, if you take paraldehyd, the last one I mentioned, an ordinary dose of paraldehyd would be four or five cubic centimetres, say, and if you started with the latest member of the series, veronal, in the same dose, it would be poisonous. The dose of veronal would be only about a tenth of the dose of paraldehyd, and it might be a very dangerous matter.

4739. Veronal is the one most approved now, is it not?—It has been introduced in the last two or three years, and is becoming very popular.

4740. (*Colonel Lockwood.*) Is it a coal tar product, made from coal tar?—No, it is not made from coal tar. It is made from the alcohol series. All those are alcohol derivatives almost.

4741. (*Chairman.*) I see among them is urethane. Is that an anæsthetic too. I think we have been told it was used as an anæsthetic?—Urethane is an anæsthetic on animals in large doses, and would be in man if sufficient were given; but it would require a very large dose in man, I take it, to cause anæsthesia.

4742. However, it is the same drug we have heard mentioned as used as an anæsthetic?—Yes, all of those drugs are anæsthetics in large doses. In small doses in human beings they cause sleep; in large doses they remove pain entirely—all this series.

4743. (*Sir William Church.*) Does an animal recover from an anæsthetic dose of urethane?—There is a large mortality I would say; but they can recover.

4744. If ever a man had a dose of urethane which acted as an anæsthetic the chances would be greatly against his recovery?—I think so.

4745. (*Sir Mackenzie Chalmers.*) Do the animals recover only under special treatment or do they recover naturally from the anæsthetic?—They recover naturally, provided they are not allowed to become too cold; but unless precautions are taken in that way they are very apt to die. A certain proportion of them would recover, I would say.

4746. (*Mr. Ram.*) Have any of these discoveries of Crum-Brown and Fraser disclosed drugs which were poisonous in themselves, which, if untested by animals, might have killed human beings?—Crum-Brown and Fraser were interested only in drawing a chemical law; they thought they would draw a chemical law. All of those first four or five bodies belong to one chemical series, and the question they tried to work out was whether all the members of a chemical series would

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act in the same way; whether, in fact, pharmacological action went along with chemical composition, was dependent on chemical composition. But, although the general rule is true, there are so many exceptions that each has to be tested in animals before being used.

4747. What I wanted to get at rather was this, with regard to soporifics generally have experiments in animals disclosed any drugs, which, if tried first in human beings, would have been dangerous, and probably fatal?—It depends upon the dose. Chloral, for example, in large doses, would undoubtedly be fatal.

4748. Then it is in the matter of the standardising of drugs that animals have been found principally useful?—There are other hypnotics which are not introduced into the series in this list. There are some 10 or 12 out of 200 or 300 compounds. Take, for instance, this last pair, veronal and propanal. When Mering and Fischer were working at those they took up at least 20 or 25 drugs and tested them on animals, and found that those two were the two that were of value. Of course, they could have tested the other 20 or 25 on themselves, but they preferred to use animals.

4749. (Sir Mackenzie Chalmers.) And moreover any one of these, any unknown drug might turn out to be a violent poison?—Yes.

4750. (Sir William Church.) The whole of these drugs are poisonous?—In large doses, and, of course, when they are invented one does not know what is a large dose and what is a small dose.

4751. (Chairman.) Then with regard to the other anæsthetics you were going to give us some evidence of the discoveries in that direction?—The second group I would mention are local anæsthetics, and of those the first would be cocain. Over thirty years had passed since the introduction of the general anæsthetics, ether and chloroform, when Von Anrep examined the effects of a new alkaloid, cocain, on animals. There was no idea of its anæsthetising effect. On the contrary, all that was known of the coca plant was its traditional power of preventing fatigue and hunger. Von Anrep noted, in the course of his experiments, that cocain induced local insensibility to pain, and states at the end of his paper that he had "intended, after the examination of the physiological action of cocain on animals, to investigate its effects on man; other occupations have made that impossible for me as yet. But I would recommend cocain as a local anæsthetic." But the idea of local anæsthesia was so new that his words passed unheeded, and I believe that he died shortly after writing his paper, and without realising the importance of his discovery. In 1884, however, Koller, an ophthalmologist of Vienna, took up the question again, and tested the anæsthetising properties of cocain on the eyes of guinea-pigs, rabbits, and dogs. He found that a few drops of a two per cent. solution applied to the eye removed sensibility to pain in the organ, while inducing no general symptoms. The eye could be touched or scratched without the least symptom of pain. "After animal experiments, which were so remarkably successful, I did not hesitate to use cocain in human eyes." He used the same solution and the same method as he had learned was successful in animals, and the result was the introduction into medicine of a new method of relieving pain and permitting of operations, whose value is recognised by every surgeon and practitioner of medicine. But while cocain caused a revolution in many departments of surgery, its use was soon found to be attended with certain drawbacks; it could not be sterilised very easily, and a number of grave cases of poisoning occurred.

4752. When you use the word "sterilised" in that sense, what does it mean?—To form a solution free from living bacteria, and cocain cannot be sterilised, because it is apt to decompose in boiling. Boiling is the ordinary method of sterilising, and cocain tends to suffer by boiling, so that new substances were looked for.

4753. Do you mean that boiling is the only method of sterilising any drug?—It is the most convenient one for the use of cocain. Substitutes for cocain were therefore sought for, which should combine its anæsthetising properties with a less poisonous action. Their toxicity had, of course, to be tested on animals of all kinds. As a result of these experiments, eucaïn was introduced by Vinci, and has since been followed by anæsthesin, novocain, acoïn, stovain, alypin, and others, which appear to be less toxic, but are still capable of much improvement.

4754. Does less toxic there mean less liable to bacilli?—No, less toxic here means less likely to poison the patient. Cocain is poisonous in itself, quite apart from infecting. Those are less poisonous.

4755. I thought the difficulty about cocain was not its poisonous nature as a drug, but the difficulty of sterilising it?—Both difficulties; there are two difficulties.

4756. How do you deal with the sterilising of these other drugs?—They do not decompose in boiling. Cocain is a rather unstable body, which tends to decompose, while the others do not.

4757. But if you asked for cocain at a chemist's now, does he sell you the cocain which is toxic and which is not sterilised, or does he sell one of these other substances?—He sells you the original cocain, toxic and unsterilised. Those others are separate drugs—those are new drugs.

4758. But oculists, for example, perform operations with the use of cocain?—Or with eucaïn, one or the other; some still use cocain, which is more powerful. It is a more powerful anæsthetic, although it is more poisonous.

4759. Then you say that other bodies besides those which you have mentioned have been discovered for local anæsthetics?—Yes, in the search for new substitutes for cocain, some other bodies—the orthoforms—were found, which, while not available for the same purpose as cocain, have a valuable effect in relieving gastric disease, and some other conditions. Those orthoforms are insoluble cocain, so to speak; they are insoluble substances which have an anæsthetising effect.

4760. Are they swallowed?—They can be swallowed. They are not poisonous, because they are not absorbed.

4761. If you are applying it to pain in a gastric disease, would you swallow it?—Yes, it is an insoluble powder. No local anæsthetic has been discovered except by means of experiments on animals.

4762. You state with confidence that that is so?—Quite confidently. None have been discovered in any other way. The whole idea of local anæsthesia is due to animal experimentation.

4763. What is the nature of these experiments that you use upon animals for the purpose of discovering whether a particular drug is an anæsthetic, and a safe anæsthetic?—In examining cocain a large number of experiments would consist in taking blood pressure, and that sort of thing. When eucaïn was introduced the first point to be made out was, is it as poisonous as cocain; if it is as poisonous as cocain, it has no particular advantage, of course, and its exact degree of poisonousness had to be ascertained on animals. A considerable number of animals were poisoned by various doses to find the smallest dose that was certainly fatal.

4764. When you give the animal the poison, do you allow it to live as long as it can to test whether it is a fatal dose?—Yes.

4765. And you judge whether it is a fatal dose by the fact of the animal dying. You do not keep it alive under the anæsthetic?—Not in those cases; not in comparative toxicity cases.

4766. Do you require a certificate for that?—You require a certificate for that.

4767. A licence and a certificate?—Yes. The third group of remedies introduced by this method are antipyretics and analgesics.

4768. What does antipyretic mean; is pyretic used in the sense of burning fever?—Yes.

4769. That would be the same thing as antifebrin?—Yes, antifebrin was one of the early ones.

4770. Analgesis is anti-pain?—Yes. Another large group of remedies which were introduced by means of experiments on animals is formed by the antipyretics, such as antipyrin, antifebrin, and phenacetin. Until 1870, the only reliable means of combating fever temperature was quinine. A fall in fever temperature in animals and man was observed soon afterwards from the use of several drugs, such as salicylic acid and kairin, but these proved to induce unpleasant symptoms, and the first antipyretic which has succeeded in maintaining its foothold in therapeutics was antipyrin, which was recommended by Filehne on the ground that it reduced the temperature in animals in fever by artificial infection.

4771. (*Colonel Lockwood.*) What do you mean by artificial infection?—The animals were injected with the hay bacillus; fever was caused in them, and then antipyrin reduced the temperature. Antipyrin was soon followed by antifebrin or acetanilid, which was recommended by Cahn and Hepp, from their results in animal experiments. They state that “we convinced ourselves by repeated and varied experiments on dogs that antifebrin, in contrast to the nearly-related anilin, can be given in relatively large doses without any poisonous action.” Examination of the changes undergone by antifebrin in the tissues of animals suggested that bodies of a certain chemical composition (paramidophenols) would also elicit this action, and phenacetin was soon introduced, to be followed by a large number of similar bodies. Each of these had to be tested in animals before being used in therapeutics, as it was found that though many paramidophenols are antipyretics, some of them are inactive, and others elicit undesirable symptoms. Among those available may be named phenacetin, analgin, thermifugin, antithermin, salipyrin, exalgin, lactophenin, malakin, saliphen, salophen, apolysin, citrophen, kryofin, phenocoll, salocoll, euphorin, thermodin. These bodies were introduced as substitutes for the earlier members of the series, on the ground that they were less poisonous, as was demonstrated in a series of animal experiments. There is no question that all the antipyretics were used to an unnecessary extent fifteen years ago. But there is equally no question that they have a great sphere of usefulness in relieving pain and discomfort and fever. All the modern antipyretics were introduced by means of animal experiments.

4772. (*Chairman.*) You stated just now that a fall in fever temperature in animals and man was observed, soon after 1870, from the use of several drugs, such as so and so. Do you mean that that was a clinical observation or was it in an observation that was made in the laboratory? Was it discovered by experiments on animals that a fall in fever temperature was observed after several drugs? How did it come about that certain animals had drugs given them which it was perceived produced a fall in temperature? What was the object of the experiment in which that was discovered?—The idea was that animal tissues might be rendered antiseptic by means of drugs. It was in the earlier days of antiseptic treatment, and it was believed that by giving salicylic acid the animal might be made to resist infection, and with the idea afterwards that in man the cause of such a disease as scarlet fever for example, might be actually destroyed in the body by giving antiseptics like salicylic acid.

4773. At that time was that experiment made with a view of discovering what the effect of antipyrin or of these particular drugs mentioned would be to produce a fall in fever temperature? You say that a fall in fever temperature in animals and man was observed soon afterwards from the use of certain drugs. The drugs were being used for another purpose I understand?—Yes, the drugs were being used for another purpose.

4774. To ascertain whether it would give them immunity?—Yes, whether they could be defended against bacilli; I think that was the object.

4775. Do you mean that incidentally in making those experiments on animals it was found that the temperature of the animal fell?—Yes, the animal was in fever, and the idea was to destroy the micro-organisms which had caused the fever.

4776. (*Sir Mackenzie Chalmers.*) That failed?—And this salicylic acid was given with that object; and although the fever was not destroyed, the temperature fell.

4777. (*Chairman.*) But was it known before that one of the effects of salicylic acid was to reduce the temperature?—No; it was known only that it destroyed micro-organisms, but it was not known that it acted upon the temperature.

4778. Now it may be suggested by others that that is a thing which could be discovered without experiment on animals?—Yes.

4779. (*Sir William Church.*) But surely it was known in 1870—they had been observing clinically—that salicylic acid reduced the temperature in rheumatic fever?—Yes; not in 1870, I think.

(*Sir William Church.*) I had a case myself at that time.

4780. (*Dr. Wilson.*) It was discovered by MacLagan, was it not?—He used it. I think the use of salicylic acid in rheumatic fever came in in 1875.

4781. (*Sir William Church.*) But salicin was used roughly for hundreds of years in rheumatism, and of course the clinical thermometer only dates from about say, roughly, the year 1860. Before 1860, during my student time, the thermometer began to be used clinically, and in 1862 I took many observations myself I remember; I was one of the few people using it; so that, as a matter of fact, salicin had been used as an antifebrile drug, had it not, for a great number of years, especially with the natives of South Africa?—I cannot go into that. It was introduced—at least, the credit for the introduction was given to MacLagan—about 1876; it was used popularly, I believe, in the Fen district, but that I cannot tell.

4782. It is of no importance, but I thought that the antifebrile effect of salicylic acid and its derivatives had been really observed first in man. That was the only point?—I think not. I think Buss discovered it, along with Furbinger.

4783. (*Chairman.*) I was asking you a question with reference to your statement that all the modern antipyretics were introduced by means of animal experiments. I was asking you whether or not the discovery had been made in clinical observation or whether it was in process of being made?—I should not include salicylic acid in that statement, because it is not a modern antipyretic. Salicylic acid is not included in the group of antipyretics and analgesics.

4784. But I understood that this discovery of the modern ones arose out of an experiment or inquiry which you spoke of as taking place after 1870?—None of the modern antipyretics were invented until antipyrin was introduced by Filehne, and I cannot give you the exact date of the introduction.

4785. Then when you say all the modern antipyretics you mean antipyrin and others, but not necessarily every drug that would reduce temperature?—No; all those that are used now to reduce temperature, I would say.

4786. What you mean is that these were tested on animals to see whether they could be safely applied?—Yes, and whether they would reduce temperature.

4787. It was not known that these particular ones would reduce temperature?—No, they have all been invented in the last thirty years. None of them were known until 1880 practically. The next group is physostigmin. The experiments of Fraser on animals first demonstrated the action of physostigmin or eserine on the eye, and led to his examining whether the same results could be obtained in man. The success attending these experiments led to the introduction of this drug into ophthalmic therapeutics, notably in the treatment of glaucoma.

4788. Is this physostigmin a remedy, or is it an anæsthetic, or what is it?—It is a remedy; it is not an anæsthetic; it is a drug. Glaucoma appears to be due to an increased pressure in the interior of the eyeball and physostigmin reduces that pressure.

4789. Glaucoma is a very painful thing, is it not?—It is said to be extremely painful. I should like to mention in reference to a question asked some time ago as to whether the examination of physostigmin might have been carried out in man, that the experiment was done in man to begin with physostigmin, or rather with the bean from which it came, by Sir Robert Christison.

4790. About when?—I am not certain.

4791. Fifty or sixty years ago, at any rate?—Yes, it proved almost fatal. Christison almost died of this experiment. He tried it without discovering that it reduced the interocular pressure. Fraser, on the other hand, adopted the safe plan of trying it on animals, and discovered its effect on the eye.

4792. Did Christison nearly die from using too strong a dose or too strong a power of it?—He did not know the dose at all. It was a new drug that had been recently brought from Africa, I think.

4793. It was from using an improper dose?—Yes.

4794. Fraser's discoveries were directed, I understand, to ascertaining what was a safe and effective dose?—Yes, and what was the action, what the drug would do, and what could be safely used.

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4795-6. (Sir William Collins.) Did he find that it contracted the pupil?—Yes, that it contracted the pupil and reduced the pressure.

4797. (Chairman.) Then what is the next group?—The fifth group would be cardiac tonics, and the first of those would be strophanthus. An African arrow poison was sent to Sir Thomas Fraser as an object of toxicological interest, and the examination of its effects in animals led him to the discovery of a valuable new heart remedy, strophanthus, which has since been used widely, and is contained in most pharmacopœias.

4798. Is that something that is swallowed?—Yes, it is used instead of digitalis in certain conditions. Several other cardiac tonics have been introduced into therapeutics, and have had more or less vogue, helleborus niger, convallaria, and so on.

4799. In these cases, too, was it discovered by experiments on animals, or was it discovered clinically, that this particular drug had a particular effect; or was the merit of experiments on animals that they showed what was a safe dose or method?—Nothing was known in regard to strophanthus when Fraser took it up; neither what it would do nor what its dose was. The whole of the knowledge that it acted on the heart and on the vessels and on the kidney, and so on, was derived from his experiments on animals, besides the knowledge of the dose.

4800. And also the testing as to the method of applying it?—Yes, besides the dose and so on. The sixth group would be vascular dilators, of which the first is amyl nitrite. An observation made by Gamgee in the course of some experiments on the effects of drugs on the blood pressure in animals formed the grounds on which Sir Lauder Brunton was induced to try it as a remedy in angina pectoris. These experiments, and those subsequently performed by Brunton and others, showed that this substance exercises a powerful influence on the walls of the vessels, and thus reduces the blood pressure. Brunton's inference was that it would reduce the blood pressure and relieve suffering in patients, in whom he had observed a very high pulse tension, as in angina pectoris. The immediate relief given by nitrite of amyl in angina pectoris has been testified to by thousands of patients and physicians, and amyl nitrite is now recognised by all the pharmacopœias.

4801. Is that something that is swallowed too?—It is inhaled.

4802. A tube is broken?—A little glass ball is broken.

4803. And that is in very general use now, is it?—Yes, it is in very general use.

4804. And you say that that arose out of observation made by Gamgee in the course of some experiments on the effect of drugs on blood pressure in animals?—Yes. The exact sequence of the discovery I should like to leave to Sir Lauder Brunton when he comes. I think this is the story I have been told, that he was present at one of Gamgee's experiments, but it may have been otherwise. Soon afterwards, nitroglycerin and nitrite of sodium were introduced on similar grounds, each having been tested on animals, and more lately erythroltetra-nitrate and other nitrites and nitrates have similarly been advocated in therapeutics. No vascular dilator has been discovered except by means of animal experiment.

4805. (Sir William Collins.) What date do you give for the introduction of nitroglycerin for this purpose?—I cannot give you the exact date.

4806. (Chairman.) Can you get it?—I cannot give you the exact date of it. If you think it of importance I can send it you afterwards.

4807. (Sir William Collins.) I shall be much obliged to you.

4808. (Chairman.) Then vascular contractors is the next group, I think?—The seventh group is the vascular contractors. Oliver and Schäfer and Szymonowicz discovered simultaneously, in the course of some experiments on animals, that the extract of suprarenal gland exerts an extraordinary action on the vessel walls. This observation was followed almost immediately by the introduction of the extract into therapeutics to induce local constriction of the vessels in inflammatory conditions and in hæmorrhage. Soon the active constituent, adrenalin, was isolated, and a solution of this body has since been used throughout

the civilised world. New uses to which it may be put are still being developed, and the importance of the discovery cannot be over-estimated.

4809. Were these experiments of Oliver and Schäfer and Szymonowicz made in this country?—Oliver's and Schäfer's were made in University College.

4810. And, I think, we have had some evidence about these experiments. When you claim that all the vascular contractors have been discovered by animal experiments, do you mean by these experiments which you have just mentioned, and others following on those?—Some nearly related chemical compounds have been recently examined by Meyer and prove to have a similar action. All the vascular contractors have been discovered by animal experiments.

4811. Were those the earliest?—Those were the earliest of those vascular contractors.

4812. About when would that be? I see you have a note, "Journal of Physiology," Volume XVIII., that would guide you?—They must have been about 1897, at a guess.

4813. Then we will pass on to diuretics?—The eighth group of drugs would be diuretics, the first of which is caffeine. Caffeine had been used occasionally in medicine, but its action and usefulness were misunderstood, as it was generally supposed to be a cardiac tonic. Von Schroeder first vindicated for it a position in therapeutics as a pure diuretic by his researches on animals, and its use has since been much extended. Von Schroeder found out that caffeine was somewhat uncertain in its action, owing to a secondary effect it exerts on the circulation, and advocated in its stead the use of the nearly related substance theobromin as a more powerful and more certain diuretic. Theobromin had not been investigated before, either in animals or in man. Since Von Schroeder's experiments, it has been used very extensively in therapeutics, either as theobromin or in diuretin and similar bodies. Urinary disinfectants and solvents form the ninth group. Nicolaier, in 1894, introduced urotropin into therapeutics, stating that "on the basis of experiments with urotropin, which indicated that only very large doses induce symptoms, and that these disappear as soon as the treatment is stopped, I thought it safe to try its effect on man." This drug was followed by a number of other remedies of similar constitution, which have been used more or less for their effect in gravel and other conditions.

4814. Were those English or foreign operations?—Foreign operations—German.

4815. Is the remedy one in general use now?—It is very largely used under various names.

4816. Do you consider it a valuable remedy?—I think its value is overestimated, but the medical profession seem to believe it is a very valuable one in general. I would not be taken as endorsing that view.

4817. You have something to say as to modifications of older remedies?—Some of the older remedies have been modified by chemical manipulations, and the products have been advocated in medicine after preliminary trials on animals. As examples may be cited heroin and dionin from morphin, euquinin from quinine, aspirin from salicylic acid, and so on.

4818. Do you mean experiments on animals or in chemistry when you say after preliminary trials on animals?—Of course the modification is carried out by the chemist; the chemist forms perhaps twenty different compounds from morphin, and sends them to the pharmacologists to investigate which of them are useful. The pharmacologist investigates their effects on animals, and selects one, or it may be two, and so on; but before they are mentioned in medicine they are all subjected to trial on animals.

4819. We now come to antiseptics?—The local antiseptic action, such as that of carbolic acid, could not be tested in animals, but only on living microbes. But as soon as it was realised that in addition to their local action they might prove poisonous by absorption into the general system they became the subject of animal experiment. And new antiseptics were advocated on the ground that they were less poisonous to the higher animals, and therefore to man. Examples are the creosols (such as lysol), thymol, and many substitutes for iodoform. The most powerful disinfectant introduced of recent years is formaldehyd, and Aronson, before advocating its use, tested whether it could be safely applied by exposing animals to its

vapours for several hours, and also by injecting it hypodermically. The twelfth group is the general anæsthetics. A number of new anæsthetics have been examined in experiments on animals, and though none of them have ousted ether and chloroform from their positions, they have enjoyed a certain limited popularity—pental, ethyl chlorid, ethyl bromid. Then we come to the emetics. In 1869 Matthiesson and Wright formed apomorphin from morphin, and Wickham Legge found from experiments on animals that it possessed powerful emetic properties when injected hypodermically. It soon found a place in the pharmacopœias, and has retained it as the most reliable and most convenient method of causing vomiting and evacuating the stomach in cases of poisoning. Finally, I would mention the preparations of the thyroid gland. But I believe that this point will be taken up by others. This list by no means exhausts the new drugs introduced by means of the experimental method in the last forty years, during which it has been systematically practised with a view to investigating the action of remedies. I think forty years is perhaps longer than I ought to state it has been used practically in the investigation of medicines; it has been used more largely in the last twenty-five years. I have no desire to minimise the importance of other methods of investigation, but when one contrasts the number of valuable drugs introduced into therapeutics without the aid of experiments on animals, one finds it disappointingly meagre. I exclude the discovery of the local antiseptics, such as carbolic acid, iodoform, and salicylic acid, which are used to act not on the patient but on his parasites, and whose usefulness could therefore be discovered only by applying it to these parasites. Apart from these, I find that in the last forty years, during which the experimental method has been so fruitful in valuable remedies, the only drug of even mediocre importance introduced by other methods is pilocarpin, a sudorific which is occasionally employed in dropsy, and which was introduced in 1874, from its being used by the South Americans as a sudorific. Another service which the method of animal experiment has done therapeutically is in the sifting out of valueless drugs. A large number of old vegetable and animal bodies which used to cumber the pharmacopœias is slowly disappearing, as the medical profession learns that they are inactive, and as the theory on which they were introduced is shown to be erroneous. A much larger number of new bodies, the result of the activity of the chemical industry, have to be examined, and accepted or rejected as they prove to be useful or poisonous. And among those accepted comparisons of their virtues have to be continuously made. So highly does the chemical industry value the aid given it in this way that its leaders are no longer willing to wait for the dictum of the university pharmacologists as formerly, but have appointed pharmacologists and erected laboratories for animal experiment at the cost of many thousands of pounds per annum. The duty of these pharmacologists is to examine the action of the new chemical products of the factory, to reject those which are useless or poisonous, and to suggest possible improvements in those which promise to be of value. In this way many hundreds of new bodies have been tested, many rejected, and some submitted to trial by the medical profession. The cruder drugs have been in many instances replaced by purer principles extracted from them, but these principles have all to be tested before they can be known to possess the virtues of the crude drug. This may be exemplified by the present position of ergot, a drug which has been in use for many centuries for its effect in causing contraction of the womb and arresting hæmorrhage in labour. Ergot has always suffered from the fact that it is uncertain in action, some extracts appearing devoid of any influence on the uterus, while others have undoubted value. Attempts have been made for many years to obtain a more satisfactory body by isolating the active principle of ergot. Within the last year a German chemist has put on the market an apparently pure substance, clavin, which he supposes to be the essential factor in ergot. This view may be tested in two ways. Clavin may be injected into women in labour who show signs of hæmorrhage, and in course of years doubtless the question as to whether the clavin is a valuable addition to therapeutics, an inert body or a poison may be determined. On the other hand, the effect of ergot on the uterus of animals is quite well known, and half a dozen experiments on anæsthetised animals would suffice to settle the question. As a matter of fact, clavin has proved quite

inert in three experiments in which I have tested it. Another direction in which animal experiments have proved of the greatest importance is in discovering and testing remedies to be employed in cases of poisoning. Poisoning occurs so rarely that in practice it is impossible to test the value of antidotes, and even when a patient recovers after the giving of an antidote, the question always arises whether the quantity of poison taken was really a fatal dose, whether vomiting or slow absorption or some other factor was not really responsible for the recovery. In animals, on the other hand, the exact dose required to kill may be ascertained, and the effect of antidotes may then be examined with accuracy. In this way many supposed antidotal measures have been shown to be valueless, or worse, through the waste of time which their administration involves. On the other hand, the value of the anæsthetics and hypnotics in convulsive poisoning has been demonstrated; the usefulness of the atropin treatment in opium poisoning, and its limitations, have been satisfactorily established; atropin has been shown to be the treatment in cases of pilocarpin poisoning, and in some forms of mushroom poisoning, and so on.

4820. Then as to the choice of animals, what have you to say as to that? We have heard what kind of animals are used to a certain extent, and in what cases. We understand that dogs and cats are used in certain classes of cases?—Yes.

4821. Guinea-pigs, and frogs, and rats, and mice?—And rabbits.

4822. And in some cases horses?—Yes, comparatively seldom, horses.

4823. And monkeys?—Monkeys occasionally.

4824. Will you just tell us shortly what your views are about the necessity, if there are to be experiments of this kind, or the advantage of using these particular animals?—Certain species are used for specific purposes, but an examination of the action of drugs confined to one species is quite insufficient to permit of inferences as to their therapeutic value. For example, theocin was examined in rabbits and found an admirable diuretic, but a few experiments on dogs showed it to be inadmissible in therapeutics through its gastric action. Rabbits and guinea-pigs are largely employed to find the general action and the toxic dose per unit of body weight. In testing whether a new drug is poisonous we would first employ these animals because they can be obtained in large numbers of approximately equal weight. The general effects on the circulation, and any narcotic effect, may also be determined in the rabbit. Cats are used specially in investigations on the actions on the stomach and intestine, and to a certain extent on the circulation. Dogs are employed to ascertain the details of the circulatory action, the way and form of excretion, and the changes which drugs induce in the metabolism. The details of the effects on the intestines and stomach, and the action on the secretory glands proper, can be ascertained only by experiments on the dog, because other animals are too small to permit of the necessary manipulations, and also because the reaction of these organs in the dog approximates most closely that of man. And when observations have to be carried on for some time (e.g., the effect of drugs on the bile secretion) the dog is the only animal available, because rabbits cannot be kept in a normal condition, and cats will not permit the necessary handling. Dogs are also very much less liable to suffer from septic infection; in fact, I have never observed suppuration in a dog from operation.

4825. Are they not treated aseptically when they are operated on?—Yes.

4826. You mean that aseptic treatment is always successful in the dog?—Yes, it is always successful. It is more successful in the dog than in man, I should say. It is extremely important that the absorption and excretion of a drug should be ascertained before it is used in therapeutics. Otherwise the physician may continue the administration while the drug is still present in the tissues, until it accumulates in sufficient quantity to induce poisonous effects. But the rate of excretion can be ascertained only in dogs.

4827. As regards demonstrations in teaching. Do you think that demonstration by experiments on animals is valuable?—I think it is very helpful to students.

4828. Do you think it is essential?—Yes, if a man is to have a thorough knowledge of the subject.

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4829. Will you tell us why you say it is essential that students should see such demonstrations?—It is essential in the same way as that a student should dissect a human body instead of reading about it, I consider. He could learn anatomy, I suppose, out of his text book, but he could not learn anatomy for the purpose of practising medicine and surgery except by dissecting.

4830. You give, I see in your paper, particular cases in which it is of importance, but do you consider it is important that they should see every class of operation, or would you limit it?—No, I think a certain number of demonstrations are necessary in order that they may see the character and the action of drugs, to show that it is not some mystic power that cures disease, but that drugs act directly upon certain organs. The actual demonstration of the effects of drugs and poisons in teaching is, in my opinion, absolutely necessary to give the student a concrete knowledge of the subject. It is quite impossible to describe the effects of central nervous stimulants, for example, in such a way as to give him a clear idea of how they differ in effect. And in the case of heart tonics, the relative importance of the different factors involved can only be shown by actual demonstration on animals. The absence of such teaching in the past has undoubtedly led to quite erroneous conceptions of the action of these drugs, and to many of them being classified together which differ in important features of their action, and are indicated in quite different conditions of disease.

4831. (*Sir Mackenzie Chalmers.*) Would the experiments which you advocate all be done under anæsthetics, and would the animal be killed before it recovered?—Yes.

4832. So that absolutely no pain need be caused to any animal by your demonstration?—Absolutely no pain is caused.

4833. It would not be necessary to allow the animal to recover for the purposes of demonstration?—No.

4834. (*Chairman.*) You know that there are many people who believe that although the law is so, it is not carried out. What is your experience of that?—That the animals must be destroyed?

4835. That they must be kept under anæsthetics during the operation, and they must be destroyed before they come to sensation again?—My experience has been that the animals under licence or under a demonstration certificate are always completely anæsthetised, and are destroyed before they recover consciousness.

4836. (*Colonel Lockwood.*) And therefore they suffer no pain?—They suffer no pain whatever.

4837. (*Chairman.*) We are speaking of operations, of course, which would be painful, and very painful, many of them, if the animal were not under anæsthetics?—Yes.

4838. Then the last head in your paper is the Standardisation of Drugs. What have you to say with regard to that?—The strength of most drugs used in medicine can be ascertained by chemical methods, e.g., the amount of morphin in opium and its preparations. These methods are not available in certain cases, notably in the group of cardiac tonics, including digitalis and strophanthus, in which the active principles are imperfectly known, and cannot be assayed quantitatively. The strength of tincture of digitalis dispensed was quite indefinite up to a few years ago, and tinctures prepared by different druggists varied as much as 1:4 in strength. In 1896 a method of standardising these preparations was brought to the notice of manufacturing pharmacologists by Houghton, the chief pharmacologist of Parke, Davis and Co. This method, which depends on animal experiments, was adopted by Parke, Davis and Co., and in the course of four to five years was copied by most of the larger American manufacturers. It has recently been taken up by a number of pharmaceutical manufacturers in this country, and on inquiry of one of these houses I am informed that, apart from their natural desire to issue preparations of definite strength, it was found necessary to standardise their products in order to be able to compete with other firms, notably the Americans. The advantage of using standardised preparations thus appears to be realised both by the pharmaceutical and medical professions. This method consists in finding the smallest dose of the preparation which induces

a certain result in animals. For example, digitalis is assayed by finding the smallest quantity required to arrest the heart of the frog in a given time. If the dose of two preparations vary as 1:2 for the frog, they will also vary as 1:2 in man. No attempt is made to derive the actual dose used in therapeutics from the dose found active in the frog by a consideration of relative weights of man and frog. In addition to the quantitative standardisation, it is necessary to perform one or more qualitative estimates, because some preparations of digitalis, while arresting the frog's heart, exercise an unfavourable influence on other parts of the circulatory system. For this purpose the experiments are performed on the dog, in which the reaction of the circulation to digitalis resembles that in man more closely than in any other animal I have examined. The activity of strophanthus and squills is determined in the same way as that of digitalis; that of ergot is found by experiments on cats, rabbits, and dogs; cannabis indica is assayed in the dog, which reacts much more exactly than the cat or rabbit; and adrenalin and products of the suprarenal gland also are assayed on the dog. The importance of standardisation of these drugs can be easily understood, and the ordinary pharmacist is quite helpless in the matter. For example, I have found that of two tinctures of digitalis supplied me by a perfectly reliable firm, the one was four times as strong as the other. If a patient were treated with the weaker for some time in the increasing doses which would be necessary to elicit the therapeutic effect, and then the treatment were inadvertently continued with the stronger in the same dose, the results might very easily be disastrous. In the case of ergot, the conditions are even worse, for much of this drug on the market is practically inert, and the preparations, of course, valueless. A physician may thus depend, in an emergency, on the action of a drug which is without action. I may remind the members of the Commission that ergot is very largely used in cases of hæmorrhage in childbirth, in which it is absolutely essential to lose no time and to be certain of one's remedies if the woman's life is to be saved. In regard to another drug, cannabis indica, I have personal knowledge that 20,000 pounds of it were offered for sale to a firm, which, before closing the deal, had a sample tested on animals, and, finding it inactive, refused the consignment. This firm informs me that since the introduction of physiological assay "we have had practically no complaints whatever as to the inefficiency of products of cannabis indica, quite a contrast to our experience previous to this time."

4839. (*Sir William Collins.*) In experimenting on animals with drugs, I suppose there are many ways in which you may apply a drug?—Yes.

4840. Such as external application?—Yes.

4841. Either to the skin or to a mucous surface?—Yes.

4842. Or a subcutaneous injection?—Yes.

4843. Or ingestion, feeding?—Yes, by the mouth.

4844. Or there may be some operation of a more or less formidable character involved?—Yes.

4845. When you speak of the use of experimentation on animals for testing the action of drugs, I suppose you include all those methods?—Yes, I include all those methods.

4846. Is not the application of any drug either to an animal or man, with a view to ascertain its action, an experiment?—Yes, an experiment pure and simple.

4847. So that it would hardly be possible to introduce any new drug except by experiment on man or animal?—It would be quite impossible.

4848. Is there any experiment on animals for pharmacological research which cannot be done under anæsthetics, in your opinion?—Certain experiments cannot be done under anæsthetics satisfactorily. For example, in comparing cocain with eucain, the point you want to find is the relative poisonousness of the two, and you have to inject cocain into one rabbit and eucain into another, and see which is more fatal. That could not be done under an anæsthesia.

4849. But is there any research of a pharmacological character upon living animals which you would require to carry out involving a considerable surgical operation, which could not be done under anæsthetics?—No.

4850. You called the attention of the Commission

to digitalis in particular. Is our knowledge of the action of digitalis, and its usefulness or otherwise, in cases of pneumonia established now?—I think it is fairly established. Its clinical facts are established. It is capable of revision.

4851. And whether the heart is affected or not is it possible to treat pneumonia successfully without resort to digitalis?—I should say that the statistics of cases of pneumonia in which the heart is affected are improved by digitalis.

4852. Would you give a reference to such statistics?—I could not give a reference to the statistics.

4853. Would you say that pneumonia in which the heart is affected cannot be successfully treated without resort to digitalis?—I should say that a certain number of cases of pneumonia in which the heart is affected will die, whether digitalis be used or not, but if digitalis be used judiciously the number will be smaller than if digitalis had not been used.

4854. When you speak of the heart being affected, something would depend upon the mode in which the heart is affected, would it not?—I believe that the affection of the heart is an intoxication by pneumonia toxin practically—the pneumo-coccus toxin.

4855. When you speak of the heart being affected you mean affection arising out of the action of the pneumonic poison itself?—Yes.

4856. You do not mean dilatation of the heart or the secondary influence of pneumonia upon the heart in that case?—I do not quite understand you.

4857. When you speak of the heart being affected, do you include in that the secondary effect of interference with the pulmonary circulation of the heart, or are you alluding exclusively to the toxic influence of the pneumonia poison upon the heart?—I do not think that clinicians can differentiate, or have differentiated, in a sufficient number of cases between weakening of the heart due to pneumo-coccus poisoning and that due to obstruction in the lungs.

4858. In any case in which the heart is affected in cases of pneumonia, would you say that digitalis would be beneficial?—I believe that digitalis is beneficial in those cases.

4859. And never prejudicial?—I really could not give an opinion. I should refer you to Dr. Mackenzie, who is the proper authority for that line.

4860. You said that the effect of recent research upon the effect of digitalis has been to show that its influence is much more complicated than was previously believed?—Yes.

4861. And I think you said that in the last quarter of a century a great deal has been learnt in regard to the action of digitalis?—Yes.

4862. Do you think that the next quarter of a century will also add more to our knowledge in that respect?—I hope so.

4863. Now, in regard to lead and opium, what did you say was the date of that research which led physicians to disuse the lead and continue the opium in cases of hemoptysis?—I might particularise the researches on the metals—I cannot put an exact date to it; between 1860 and 1875, I should say.

4864. Would you say that it was the practice up to relatively recent years for well-informed physicians to prescribe lead with opium for hemoptysis?—I have seen it done by well-informed physicians.

4865. And avoided by others?—And avoided by others.

4866. Have chloral and sulphonal dangers as soporifics?—Yes, every soporific has more or less danger; chloral and sulphonal have greater dangers than some of the more modern ones, I think.

4867. What was about the date of the introduction of cocain as a local anæsthetic?—Von Anrep's research was published in Pflüger's Archives, so far as I remember, in 1880.

4868. But it was after Koller that it was brought into use in 1884?—Yes, in 1884.

4869. Had there not been a series of researches into the physiological action of cocain and other similar alkaloids by Hughes Bennett as long back as the sixties and seventies?—Not on cocain that I remember.

4870. Do you not remember some researches published in the "Edinburgh Medical Journal" of 1873, by Hughes Bennett?—I know that he performed re-

searches, but I did not think that he included cocain. I would not express a certain opinion upon it.

4871. You do not happen to know that he used frogs, mice, rabbits, etc., to try the effect of cocain at that date?—It is very possible.

4872. Did he discover the use of cocain as an anæsthetic?—Not that I remember.

4873. Have you heard from Koller how he discovered it?—No. I have read his original paper. I do not know Koller personally.

4874. Did he, or did he not, chew the leaves and find an anæsthetic influence on the lips?—He says nothing of that in his paper.

4875. You have not had an opportunity of personal conversation with him?—I do not know him personally at all.

4876. Was any surgical operation necessary on animals in order to establish the use of cocain as a local anæsthetic?—Koller states that he applied the alkaloid to the dog's eye, and then touched, scratched, and burnt (three terms I remember) the cornea in order to determine whether the animal was sensitive.

4877. Would the application of a solution of cocain to a mucous surface result in anæsthesia?—Yes, in anæsthesia as established by burning the mucous surface.

4878. By various forms of irritation?—Yes.

4879. In regard to the group of antipyretics, did I correctly understand you to suggest that antipyrin has replaced salicylic acid and quinine as an antipyretic?—It has replaced quinine.

4880. Do you mean that quinine is not used nowadays as an antipyretic?—It is not used to the same extent as it was forty years ago. It is used in malaria, but not as an antipyretic or an analgesic.

4881. Should I be wrong in thinking that quinine is nowadays prescribed for the purpose of reducing temperature?—Not very largely; it may be occasionally—not very largely in non-malarial disease, I should say.

4882. Do you practise as a physician yourself?—No, I do not practise.

4883. Then when you speak in your pamphlet of all modern antipyretics having been introduced by means of animal experiments, do you mean by modern antipyretics those that are nowadays employed?—Yes.

4884. Then are you satisfied that salicylic acid and quinine were brought into use as the result of experiments upon animals?—No; I think I attempted, in answering some questions before, to limit that phrase. The group of antipyretics and analgesics is a perfectly well defined one, and does not include either quinine or salicylic acid. It includes antipyrin, antifebrin, phenacetin, and a score or two more similar bodies; but it does not include either salicylic acid or quinine. Quinine, of course, is a drug that has been known for 170 years, getting on to 180 years.

4885. But you can hardly say, can you, that salicylic acid and quinine are not nowadays used to reduce temperature?—I think I am correct in saying that salicylic acid is not used to reduce temperature except in rheumatic fever—acute rheumatic fever.

4886. And does it reduce temperature there?—It reduces temperature, but by a specific effect upon the organism, upon the cause of the rheumatic fever. Those antipyretics, like antipyrin and antifebrin, are defined by reducing the temperature without affecting the cause of the temperature.

4887. Do we know of a micro-organism as the cause of rheumatism?—We suspect one, at any rate.

4888. Could you speak with any certainty there?—I cannot speak with any certainty there, because I am not a bacteriologist.

4889. With regard to physostigmin, I understand you to say that Christison, although it was rather a serious experiment, had established the fact that contraction of the pupil resulted from the application of a solution of the Calabar bean to the eye?—I did not mean to say so. I said that Fraser established it, while Christison examined the action on himself, and failed to recognise that it was of use in glaucoma.

4890. But did Christison not find that it contracted the pupil?—I believe he did, but I am not quite sure of that; but it was almost at the expense of his life. I should not like to state definitely at present as to that.

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4891. Is not its influence in reducing the tension of the eye consecutive upon its action in contracting the pupil?—It accompanies contraction, but whether it is consecutive I believe is still disputed.

4892. Would the tension fall if the contraction of the pupil did not take place?—I cannot express any opinion upon that.

4893. Then as regards diuretics, caffein, thein, and theobromin, should I be wrong in thinking that the diuretic influence of coffee and tea was established before these more recent experiments with caffein, thein, and theobromin to which you have referred?—I do not think it had been established; at any rate, it had not been established in medicine. No medicinal use, or a very slight medicinal use, had been established before Von Schroeder's work. I am not aware of any statement that they had been used in medicine as diuretics.

4894. As regards amyl nitrite and its use in angina pectoris, you referred to Sir Lauder Brunton, I think, as the discoverer of that use?—I declined to decide whether it was Gamgee or Brunton, but they discovered it between them. Whether Brunton observed it under Gamgee, or made an independent experiment I should not like to say.

4895. Have you heard the name of Sir Benjamin Ward Richardson in connection with that?—Not in connection with its action upon blood pressure.

4896. You refer in a footnote to the "Lancet" of July 27, 1867, as the authority for Sir Lauder Brunton's research?—Yes.

4897. Do you happen to know that in the "Lancet" of that date reference is made to Dr. B. W. Richardson's previous experiments?—Not on the blood pressure, I think—not to experiments on blood pressure.

4898. Are you sure of that?—I am never sure of anything unless I have it before me, but I am practically sure in my own mind.

4899. Did he not state that it had the effect of relaxing the muscles?—It is very possible; it is some time since I read the paper.

4900. Did not Dr. B. W. Richardson state that it was brought to his notice by Guthrie, a chemist?—I do not know that. I may state that this paragraph in my *précis* was read by Sir Lauder Brunton, and has his general approval.

4901. (Sir Mackenzie Chalmers.) He is coming as a witness?—Yes, he is coming before the Commission.

4902. (Sir William Collins.) As a matter of history, have you read in Sir Benjamin Ward Richardson's work "Vita Medica" the statement in regard to his research into nitrite of amyl: "I proceeded in the usual way. I made myself the first victim, and tried the effect of the substance by taking it both in the form of vapour and fluid"?—I cannot say that I am very familiar with Sir Benjamin Ward Richardson's work.

4903. I think you have alluded to the use of thyroid extract for myxedema or some product of the thyroid gland, have you not?—Yes, I have. As regards the thyroid extract, I also refer to future witnesses.

4904. Was that discovered by vivisection experiments on animals?—I would refer to Sir Victor Horsley upon that point, who is the authority.

4905. You would rather not speak of your own knowledge upon it?—I would rather refer to him.

4906. As a pharmacologist, what would you say as to the inferences to be drawn from the effects of drugs on animals in regard to what effect they will have upon man?—I should say that any inference is to be drawn from experiments on animals with the greatest care, and after much experience. If a young experimenter goes and injects a drug into an animal and sees certain effects, and immediately draws from that the conclusion that it will have this effect on man, he is liable to a considerable amount of error. If a pharmacologist of experience makes experiments he can draw an inference with some probability, at any rate, that certain effects will occur in man.

4907. But are the effects upon the animals always of the same kind or in the same direction as they are in man?—They are not always of the same degree.

4908. Do they not often differ in kind? Take the influence of morphia upon frogs, for instance?—They differ in degree, and also to some extent in kind.

4909. Does morphia produce spasms in frogs?—In large doses.

4910. Does it in man?—Man dies from the failure of respiration.

4911. Does it produce spasms in man?—No, it does not produce spasms in man.

4912. You spoke of theocin, I think, yourself, as having a different action in rabbits and in dogs?—Yes, because the rabbit cannot vomit.

4913. But does it produce irritation of the stomach in a rabbit?—It does in a dog. I should not like to say whether it produces irritation in the stomach of a rabbit or not. I have not examined it.

4914. Take the case of hemlock. Have you investigated the effects of hemlock on animals and man?—Yes, on animals.

4915. Is it a poison to all animals?—It is a poison to all the animals I have investigated it on.

4916. To birds?—I think so.

4917. Have you not come across statements to the contrary in therapeutic works?—I should not like to say that I have come across statements to the contrary in well-known therapeutic works.

4918. Do I take it from you as a pharmacologist that hemlock is universally a poison to the lower animals?—So far as I know, it is to vertebrate animals.

4919. (Colonel Lockwood.) To rabbits?—Certainly to rabbits.

4920. (Sir William Collins.) You spoke of demonstrations to students of the effects of drugs as essential, and, I think, as absolutely necessary?—I think so, to the complete education of students.

4921. Is it not possible for eminence as a physician to be attained without witnessing experiments upon animals?—I think so.

4922. It is or is not possible?—It is quite possible.

4923. Do you remember that the late Sir Thomas Watson informed the previous Royal Commission that he had never himself witnessed an experiment on animals?—It is very possible.

4924. Then in what sense are we to take it as absolutely necessary or essential?—It is absolutely necessary for a knowledge of pharmacology and therapeutics.

4925. Then is not a thorough knowledge of pharmacology essential to a well-informed physician?—For a well-informed physician, yes. For a well-informed physician or for a physician, it is simply a matter of degree. You can have a well-informed surgeon who has not dissected the human body, but the best-informed surgeon has dissected the human body, and the best-informed physician has witnessed the effect of drugs upon animals.

4926. But for the average student would you regard it as essential and absolutely necessary that he should witness these experiments on animals?—I should give every student the opportunity of becoming a well-informed physician.

4927. I understood that you did not dispute that Sir Thomas Watson was a well-informed physician?—I said nothing of Sir Thomas Watson.

4928. You would not wish to give an opinion?—I give no opinion.

4929. (Sir William Church.) When Sir Thomas Watson was alive did pharmacology exist as a science?—I am not aware, to tell the truth, of the date of Sir Thomas Watson's death.

4930. It did, perhaps, exist at his death, but not when he was a young man, did it? There was no such department in any teaching body?—There was no such department really. Pharmacology is the creation of the last thirty years—thirty years is the extreme limit.

4931. (Colonel Lockwood.) You have made a good many experiments, have you not, on living animals?—Yes.

4932. And, of course—I need not ask you—in all those experiments you are sure that the animal never suffered?—I am quite satisfied that no animal suffered pain under my experiments.

4933. Have you ever attended any other experiments by other people?—Yes, very frequently.

4934. As an interested observer?—As an interested observer, and also as the superintendent of a laboratory.

4935. And in all those experiments which you have witnessed you have never seen pain or needless suffering?—I never saw pain or needless suffering.

4936. And the animal was invariably put to death according to the terms of the licence before it recovered sensibility?—Yes. My experience of the working of the Act is a comparatively limited one, I must say, because I have only worked in this country for about two years.

4937. How many experiments have you witnessed, roughly speaking?—I suppose going up to 100.

4938. And during those experiments you have never seen anything which could have caused needless suffering to that animal?—No.

4939. Do you consider that there is enough resemblance between human beings and animals to make the experiments of which you have spoken of value in pharmacology?—Yes; I have tried to point that out in the matter of those new drugs which have been introduced above.

4940. But surely you can give many a drug—any drug, I was going to say—to a dog that you cannot to a human being without deleterious effects?—I would not say any drugs; I can say some drugs.

4941. Most drugs?—Some drugs, I would say.

4942. Therefore, there is not a complete resemblance between the action of drugs on a dog and that on a human being?—There is a resemblance to a trained eye—probably not to an untrained eye.

4943. You think there is an invariable resemblance to a trained eye of the effect of a drug on a dog to that on a human being?—Yes, I think so. I think it is fair to say that.

4944. Do you maintain that the action of digitalis could only have been discovered by experiments on living animals?—I think so.

4945. Can you suggest any alteration in the present system of granting licences, or are you content with the present system as invariably wisely exercised and humanely exercised?—I think so. The only criticism I have to make of the system is that there seems an unnecessary difficulty in the licensees getting certificates. I mean to say that the certificate is too defined. For example, I have a number of certificates which permit me to inject drugs into rabbits, cats, and frogs, and now I have had a message sent to me the other day from the Lister Institute that they want me to examine a drug, which they have found to be poisonous, in rats. In order to do this I have got to go and get another certificate from the Home Office to allow me to inject it into rats.

4946. In short, you want more licence for experimenters than exists at present? You want a freer hand, so to put it?—I think the Home Office might allow the head of a department to enjoy the use of licences without more ado instead of having so much correspondence.

4947. (*Sir Mackenzie Chalmers.*) You think that the licence ought to cover certificates, in fact; you think that once a licence is granted the certificates ought to follow?—I think it should be possible for certain licensees, say experienced licensees, to have all the certificates thrown in.

4948. (*Colonel Lockwood.*) Then do you think that pharmacology, to take the subject in which you are specially interested, has suffered from the restrictions imposed on it in England by the authorities?—I really cannot say.

4949. You would rather not give an opinion?—I would rather not.

4950. Do you or do you not think that the habit of closer clinical observations suffers rather from the fact that a student thinks he can gain the knowledge required in an easier way by witnessing experiments on the living animal?—No, that has never occurred to me at all. I should put it the other way—that a student learns in his pharmacological class the detailed action upon animals, and then he goes to the bedside and sees whether he can follow those observations in the human patient. You see the experimental part comes before the clinical.

4951. That is the way you would put it?—It really induces him to look more carefully than otherwise.

4952. Therefore, you think that experimenting on

living animals leads to more careful clinical observation rather than to less?—Yes, certainly.

4953. Would you continue—as you rather hinted that you would—to show by demonstration over and over again the well-known effects of certain drugs on living animals?—Yes, with that express intention of drawing the student's attention. You can make them perfectly obvious to a student on an animal; then he goes to the bedside and sees whether he can make out those actions occurring in his patient.

4954. Do not you think that the effects of certain drugs are so well known that they do not require continual demonstration on living animals?—Exactly. I should not illustrate those effects; but it is those effects which are not so universally recognised, or whose signs in the human subject have to be sought for more carefully, that I should demonstrate.

4955. You, therefore, rather qualify your answer. You would not demonstrate at all lectures over and over again well-known experiments; it is only certain experiments that you would show?—Yes.

4956. You know, I suppose, naturally of all the opposition that there is to what is called vivisection?—Yes.

4957. Do you believe that that is founded entirely on misconception on the part of sentimental or well-wishing people, or how do you account for this opposition?—The opposition, I think, has a certain amount of sentiment about it. I do not think there is any ground for the accusation of cruelty, but there is ground for what is really the origin of the whole thing, namely, the fear of blood. I think it is a sentimental fear of blood; that is to say, that those people have exactly the same shuddering sensation when they see a butcher's shop, who have this shuddering fear of blood and of flesh, and so on, and they imagine that our laboratories are filled with those things and with the shrieking of animals, and accordingly they raise their voices against us.

4958. You think, then it is purely and entirely sentimentalism?—It is a misconception.

4959. Do you think that there has never been any cruelty practised on animals?—I should not like to say that, but I think that the present outcry arises from misconception.

4960. Do you think that the outcry which was raised originally, and which resulted in the passing of the Act, was also entirely a misconception?—I should really not like to give an opinion about that. I was not interested in the subject at that time.

4961. At any rate, you think that people who now make these statements about what goes on in laboratories and lecture rooms are absolutely and entirely wrong from start to finish in their statements and in their books?—Many of their statements are entirely wrong. I have no doubt of that at all.

4962. But would you say that the whole of them are?—I should say that the statement that cruelty occurs in laboratories in England at the present time is grounded on misconception, and is false.

4963. (*Sir William Church.*) In physiology and pharmacology, can any experiment ever be said to be complete?—No.

4964. At any time the results obtained may lead to problems which require further investigation?—They almost invariably do so.

4965. Therefore, although some of the effects of certain drugs which have been experimented with are well known, it is quite possible that at any time further researches into them may lead to our knowledge of new properties?—Yes.

4966. Is it possible to scientifically observe the action of drugs upon a sick person?—With some drugs I should say that it can be perfectly well observed. Those drugs have all been observed. For example, the effects of purgatives are sufficiently obvious.

4967. Is it possible, even in the case of purgatives, to observe scientifically the action of a drug on a sick person? You only know that you give the drug and the patient is purged, but can you find out from that sick person how it comes about that he is purged?—No, of course not; but many effects cannot be examined even as far as that. We do not know what happens, although the patient gets better.

4968. Therefore a scientific knowledge of how a drug acts is an entirely different matter from a clinical

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knowledge of the results which it produces?—Yes, a scientific knowledge in regard to the action of a drug may indicate further uses for it.

4969. If we had simply depended on clinical knowledge we should say, "This drug makes this patient well," and therefore we would use it on the next patient?—That is exactly what has happened for centuries and centuries until this last century.

4970. But we do not know in the least from that how it acts—through what means it acts, do we?—Very seldom.

4971. Neither is it possible, on a sick person, is it, to apply scientific instruments to see whether the blood pressure is altered, and how much? It is possible, perhaps, to keep a register of the temperature from hour to hour, but even that is distressing to the patient more or less, is it not?—It is quite impossible to make most scientific investigations in that way.

4972. Now, with regard to the use of digitalis, of which a great deal has been said to-day, you mentioned its use in pneumonia; but is there not another disease in regard to which our present knowledge of digitalis has been, perhaps, of even greater importance? Would it be good treatment now, according to our present knowledge, to give digitalis in cases of apoplexy?—Certainly not.

4973. Formerly, was it very much used for apoplexy?—Yes.

4974. Of course, its tendency would be to increase the hæmorrhage?—Yes.

4975. And that is what you wish to avoid?—Yes.

4976. Therefore it would now almost be considered malpractice to give digitalis in cases of apoplexy?—Yes. I might have added apoplexy to aneurism in my statement. If I had put apoplexy and aneurism it would have been all right.

4977. You only mentioned lead and opium, I presume, as an instance?—Yes, only as an instance.

4978. Because as a matter of fact, of course, a great many people never used lead for pulmonary hæmorrhage for years and years before you mentioned it?—A great many use it at the present day.

4979. But a great many did not then?—Yes. I did not mean to cast any reflection upon the profession, of course.

4980. With regard to local anæsthetics and anæsthesia, do not you think there is a great future for local anæsthesia yet?—I think its uses are not by any means exhausted yet.

4981. But, even with such local anæsthetics as we are now acquainted with, is it not the case that what may be called quite major operations are now being performed?—Yes, and they have been for 10 years. I think every operation that I know of is being performed under local anæsthesia now, and its use, of course, is always extending.

4982. And there can be little doubt that local anæsthesia is safer than general anæsthesia?—It has many advantages besides being safer. It is safer in many ways.

4983. There is always risk of death, is there not, under general anæsthesia?—Yes.

4984. And the probability is that as our knowledge of local anæsthetics improves, they will become more and more useful?—Yes.

4985. I know you are not in practice, but could you tell the Commission some of the most formidable operations which are at the present moment done with local anæsthetics?—Almost any operation has been done. The abdominal cavity, for example, is opened, the ovaries are removed, the intestine is sutured, and amputations of legs and arms have been performed under them.

4986. I have in my mind especially those abdominal operations?—Yes, they are quite frequent.

4987. Are they a sample of what you may call severe serious operations that are done every day now with local anæsthetics?—Yes.

4988. How is it that the patients do not suffer torments from their intestines and abdomens and other organs in the abdomen being handled?—Because the evidence goes to show that the intestine and other abdominal organs have really very little ordinary sensation of pain.

4989. Is it probable that the abdominal organs and intestines of other animals than man are more sensitive?—Not at all.

4990. Just one question with regard to antipyretics. I do not quite agree with one statement you made. I do not think I quite understood you. You commenced by saying that until 1870 the only reliable means of combating fever temperature was quinine. You do not really mean that, surely?—Perhaps I should have said the only reliable drug.

4991. That is very different?—Yes. My evidence refers exclusively to drugs practically. I ought to have said drug.

4992. I would not take exception to drugs, but I did take exception to that statement?—Of course.

4993. Cold is a much greater one?—Yes.

4993a. And in the same way I do not know that I quite understand your statement about cardiac tonics. You say that several other cardiac tonics have been introduced into therapeutics and have had more or less vogue—helleborus niger, convallaria, etc. But those have not been introduced since stropanthus or since pharmacology?—No.

4994. What you really mean is that their true action and the active principles in them have been investigated?—Yes. Of course helleborus in particular is a very ancient drug. I really mean their active constituents.

4995. Both of them have been used since classical times?—Yes.

4996. What you mean is that in the case of those drugs no one knew exactly how they acted?—Exactly.

4997. Or what was the active principle in them?—No.

4998. We have heard a great deal in this room about curare, and I should like to ask you some few questions as to it. It is accepted as a maxim, I might say, that it paralyses the voluntary muscles and does not affect sensation; that has been the ordinary belief of the lay mind—they have accepted that as the result of the last Royal Commission, which decided that it should not be used as an anæsthetic?—Any ordinary dose of curare, any ordinary amount of curare, such as is used to throw the muscles out of action, I think, has not any effect at all upon the central nervous system to paralyse sensation. What is the effect in very large doses, I think, is not quite determined.

4999. Is it the case that some of those who were best acquainted with drugs of that sort doubted whether Claude Bernard's statement was absolutely true at the time when it was, we will not say accepted by the Commission, but at the time they acted upon it?—I should like the statement to be more definitely before me.

5000. Do you agree with what Sir Lauder Brunton said when he was examined? The question was put to him: "We have had a great deal of evidence before us showing difference of scientific opinion as to whether it (that is curare) is an anæsthetic, but, on the whole, the balance of evidence, in the view of Claude Bernard, seems to show that while it stops muscular motion it does not banish pain"; and Sir Lauder Brunton said: "I think Claude Bernard is mistaken." Then he goes on to explain, and say very much the same as you do—that he thinks it is a matter possibly of the quantity, and he goes into the matter at greater length than I need; but I should like to know whether your opinion is that we have positive evidence that in small doses it only acts as a paralyser?—Yes.

5001. And you do not think we have positive evidence that in large doses it acts as an anæsthetic?—We have not positive knowledge as to what effect very large doses produce.

5002. So that you have no special information that you can give us on that point?—I think there can be no doubt that with small doses it is not an anæsthetic.

5003. I ought, perhaps, to put this question to a physiologist rather than to you, but do you know whether curare is much used by experimenters in this country?—I do not think it is much used anywhere. To tell the truth, it is very difficult to get, and the reason why I refuse to give any definite statement as to curare is that it is so indefinite. We have not the same curare that we could have got thirty or

forty years ago. As a matter of fact, at the present time much of the curare that we get fails to paralyse muscles or anything.

5004. I was going to ask you whether it was not difficult to obtain, but you have already answered the question?—I tried to get it in a number of places a few years ago, and I could not get any curare that would paralyse the muscles at all.

5005. It is extremely difficult to get now?—I cannot get any.

5006. And what you can get is very expensive?—What is called curare is very often quite inactive.

5007. (*Sir Mackenzie Chalmers.*) Is it known what it is?—It is the old South American arrow poison.

5008. Is it known from what vegetable it is drawn?—It is one of the strychnos genus.

5009. But the plant itself has never been obtained here?—I do not think it is known definitely.

5010. (*Sir William Church.*) At present the only source from which it can be derived is the poisoned weapons of the natives?—Yes.

5011. All that I wished to elicit was whether you were or were not prepared to give any distinct information as to its use as a general anæsthetic, and also whether you know that it is rarely used in this country, and that it is very difficult to obtain, so that it cannot be in general use?—It is never used at all in this country without another anæsthetic.

5012. There is one question I should like to put to you arising out of what Colonel Lockwood asked you. Should you feel justified in giving a man a dose of an unknown, or only partially known, substance which had proved fatal to a dog?—Certainly not.

5013. Therefore when trying an unknown substance a prudent man would, at all events, see how big a dose an animal would bear before he went on to give any dose to a man?—Yes.

5014. And there are physiologists and pharmacologists who have a certain rule, have they not, for judging of the probable amount that can be taken by one animal as compared with another?—I would hardly dignify it by the name of a rule. One takes the body weight, and then if one is going to apply it to a patient by the body weight I should divide the result by 10.

5015. So that as a rough rule it is found that the amount of drug tolerated is more or less in proportion to the bulk of the body?—Yes. You must use the rule with great discretion, I think.

5016. Regarding that, apart from experiments, you have no way of telling?—We have no way of telling at all.

5017. (*Sir Mackenzie Chalmers.*) You have been for two years, I think you said, in University College?—Yes.

5018. But you have been studying pharmacology for a much longer time than that?—I have been teaching it for fourteen years.

5019. Where have you been teaching it?—In the University of Michigan, in the United States.

5020. I suppose in the United States any laws as to experiments on animals are State laws and not Federal laws—each State would have its own law?—I do not think there is any law on the subject in the United States, so far as I can remember.

5021. So far as you know, in America experiments on animals are not regulated by law?—No.

5022. And in Michigan, at any rate, they were not?—There was no law.

5023. Did you find any abuse there?—I never saw any abuse of animals in Michigan.

5024. But it would be unlawful, I suppose, for students to operate in Michigan?—Not at all. A student very often came to me and said: "I am interested in such and such a thing, would you mind my doing experiments in your laboratory?" I would discuss the matter with him, and if it was a likely line of investigation, I simply gave him permission to go on, showing him exactly the methods of anæsthetising, and exactly the method of operating, and so on; and very often, of course, operating with him.

5025. Was it not liable to abuse? Did you not find students on their own account doing operations that were unnecessary, and possibly painful?—I do

not think so. They may have done so; we did not find it.

5026. Would you be likely to have heard of it if they did?—There was no reason for their doing it. They could come to my laboratory, you see, and do anything reasonable, and I think public spirit would have forbidden it there.

5027. Public opinion among the students would have been against what I may call painful experiments?—Distinctly.

5028. Putting quite generally what you have said, am I right in drawing this inference that, allowing for necessary corrections, you can infer the effect on man of any drug from its effect on the lower animals?—Yes.

5029. Do you find that your experiments on animals were vitiated at all by any idiosyncrasy in the animal—that you might experiment on one dog with any drug you like and the effect on another dog would be different?—Not more so than in man.

5030. But as much so, would you say?—No, I do not think as much so.

5031. In man the difference is very marked, is it not?—There are not statistics of so many dogs as one has of man; for example, 99 men out of 100 can eat strawberries without difficulty and the hundredth man cannot; we seldom have 99 experiments in dogs.

5032. You do not think there is any danger of being misled, then, by experiments on animals owing to the idiosyncrasy of the animal?—It would be very slight.

5033. And, at any rate, a greater number of experiments would correct that?—They would, of course.

5034. May I take it generally that the result of your evidence is that the necessity for experiments in pharmacology is to find out, not merely what we may call the action of drugs, but the reason of their action?—Yes.

5035. So that you no longer give a drug empirically, but scientifically?—We try not to do so; that is the object.

5036. Take chloral, for instance. Is it not supposed that when chloral is swallowed the substance is decomposed, and that its soporific effect is due to its decomposition?—No, that was the original view of Liebreich, but that has been given up for many years now.

5037. I asked you the question for this reason: Would experiments tend to prove or disprove that?—It could practically only be proved or disproved by experiments, I should say.

5038. And for clinical purposes is it desirable to prove or disprove a theory of that kind?—I think it is desirable to cut off any roads leading to error by clinicians in any way.

5039. Have you paid any special attention to anæsthetics?—I have used a large number of them.

5040. And are you satisfied that the anæsthesia of animals is as complete as that of human beings?—Do you refer to ether and chloroform?

5041. Take ether and chloroform first; or take the A.C.E. mixture, which is very often used for animals. Are you satisfied that the animal is fully anæsthetised by those means, or would you say that it is possible that the animal can suffer pain?—I am quite satisfied that the anæsthesia is complete.

5042. Is it easy to administer to an animal?—It is quite simple—all three of them. It is very like administration in man.

5043. You say you are satisfied that in the physiological experiments that you have seen, the animal has been kept completely anæsthetised?—I am quite satisfied.

5044. Do you think that, in view of the feeling that people have about experiments, that the particular anæsthetic should be prescribed, or do you think that should be left to the operator?—I should deprecate any particular anæsthetic being prescribed.

5045. Is there a large range of anæsthetics used in experiments on animals?—I have used half a dozen to a dozen different kinds in the course of my experience.

5046. Do you think it is justifiable to use local anæsthetics for animals for anything like a severe operation?—I think it would be liable to defeat its

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object. I mean to say that the animal would probably be frightened.

5047. Therefore you would say that general anæsthetics should be used for animals?—It is possible, of course, for certain purposes that local anæsthesia might suffice; but for ordinary surgical operation I think that general anæsthetics ought to be retained and used. By general anæsthetics I do not mean merely ether and chloroform.

5048. Do you include urethane?—Urethane and paraldehyd.

5049. Are you satisfied in the case of urethane that it is sufficient?—For certain animals there is no question about it, that it is quite sufficient. For rabbits, in particular, I have had a large experience with it.

5050. Are they more susceptible than other animals to urethane?—I have not used urethane very much on other animals.

5051. But you are satisfied that complete anæsthesia is produced by it?—Yes, quite complete.

5052. Would you use urethane when you want the animal to recover from the anæsthesia, or only when it will be killed at the end?—I should use it only when the animals are to be killed, because one gives such a large dose.

5053. You give a poisonous dose, do you, practically?—There would be 50 per cent. chance of the animals dying at least, probably more.

5054. From the action of the anæsthetic alone?—From the action of the anæsthetic alone.

5055. How does urethane kill?—It stops the respiration. But in all those anæsthetics like urethane the action deepens as the animal lies anæsthetised; the anæsthesia becomes distinctly deeper, and then finally the animal dies.

5056. And that does not interfere with the experiment so long as life is maintained?—No. There is no possibility of the animal coming to at all, the anæsthesia gets deeper, and the animal seems to lose resistance to it.

5057. And you have an additional security against pain in the case of an animal, because you can push the anæsthesia further than you would do in a human being?—It almost always is so. The anæsthesia in animals is practically always what a surgeon calls deep anæsthesia—such anæsthesia as he wants when he is going to reduce dislocation of the hip, for instance; not the anæsthesia when he is going to do some minor operation on the hand or something like that, but it is such anæsthesia as when the muscles are relaxed.

5058. There was one remark of yours that I did not quite understand as regards abdominal surgery. You said that the intestines themselves were not sensitive?—They are not sensitive to pain as the hand is, for example—the skin of the hand.

5059. They are not sensitive in the same way as the external skin you mean?—That is so.

5060. What accounts then for the very great pain in colic and cholera?—It is not the same sort of pain; it is a pressure pain. I believe it is a different form of sensation altogether.

5061. It is cramp, in fact?—Yes.

5062. Then if there is an absence of sensory nerves, how is it that you have intense pain in the cramps of cholera? Which are the nerves that suffer from the cramps?—I think opinions differ on that point a little. Some authorities hold that the suffering is not in the abdominal cavity at all, but outside it.

5063. That it is only subjective sensation at the particular spot?—Yes.

5064. (Dr. Gaskell.) You are referring to Head's work with regard to referred pain?—Yes, and Mackenzie's too.

5065. (Sir Mackenzie Chalmers.) Just like a man who has had his leg cut off and fancies he feels pain in his toes; is that it?—No, it is not exactly that.

5066. (Dr. Gaskell.) Is it not a fact that all the visceral tissues have pain over certain skin areas which can be mapped out in connection with each of the viscera?—Yes.

5067. And that referred pain is the main pain usually felt in visceral disease?—Yes, but that pain, of course, does not occur on touching the skin or any-

thing of that kind, it is a different sort of pain altogether.

5068. (Sir Mackenzie Chalmers.) The British Pharmacopœia contains a vast number of substances?—Yes.

5069. Have they all been worked out, or does a great deal of work remain to be done?—The Pharmacopœia has not been worked out; there are experiments which yet remain to be done.

5070. So that if experiments were stopped now a great deal of work would be stopped in the working out of the British Pharmacopœia?—It is not only in the Pharmacopœia, but it is in the working out of the principles of new drugs that are introduced.

5071. New chemical compositions you mean?—Yes.

5072. They all have to be tested?—Yes, they all have to be tested in man or in animals. The question is whether they shall be tested on man or by experiments on animals. There is no other way of getting at how they act.

5073. You referred to caffein. I suppose caffein is the active principal in both tea and coffee?—Yes.

5074. And, I suppose, the experiments on caffein have some bearing really on the practical use or abuse of tea and coffee as well as their medicinal conditions?—Yes, still I am afraid that people do not consider pharmacology very much when they drink teas and coffees.

5075. I suppose from these experiments you will know more about the actual use or abuse of tea and coffee?—Yes.

5076. I think you said that caffein was a pure diuretic?—No, it is not a pure diuretic. It acts upon the brain, but its diuretic quality was not recognised until Von Schroeder's time.

5077. Only its exciting qualities?—Yes, only its exciting qualities.

5078. (Mr. Tomkinson.) Do you claim that practically every valuable discovery of the effect of a drug has been made through experiments on animals?—In the last forty years.

5079. And yet on page 19 of your paper you show how differently the same drug may affect two different animals, as rabbits and dogs in the case of theocin?—Yes.

5080. You say it is an admirable diuretic in one, but yet quite unsuited for the other?—Yes.

5081. Where is the analogy, then, between the effect upon an animal and upon a human being?—It is an admirable diuretic for a dog or a human being; but, in addition, it causes irritation of the stomach, which was not recognised in the rabbit, because the investigator did not examine the stomach. In the dog it was brought forcibly to his attention by the fact that the dog vomited. In the human being it also causes irritation of the stomach.

5082. By experimenting on one animal you might be led to the conclusion that it would be injurious to the human being?—Yes.

5083. And by experimenting on another kind of animal you might be led to the conclusion that it might be beneficial?—That is so, and therefore a drug should be tested on as many different kinds of animals as is possible.

5084. That somewhat weakens the contention, does it not, that every discovery which is beneficial to the human frame has been discovered through experimenting upon an animal?—It is not a contention; it is a fact.

5085. It makes it experimental to a certain extent?—No one says that a drug can be used in large quantities in a human being simply from experience of its action on animals; but its action on animals often gives a suggestion for its use, or at any rate indicates how poisonous it is.

5086. What do you mean when you state that the dog is the only animal available for certain experiments, because rabbits cannot be kept in a normal condition and cats will not permit the necessary handling? Why do you say that cats will not permit the necessary handling?—Cats scratch more than dogs do as a matter of fact.

5087. You mean that they resist more?—They resist more, and then a cat lies about much more than a dog on its belly, and so on, instead of lying on its

side, and that is often awkward. Then a cat has fur, which becomes unclean or sucks up fluid and so on, whereas a dog does not do so nearly as much. And then there is the practical fact that a cat will not stand still.

5088. What class of experiment is it in which a cat is less suitable than a dog?—Where observations have to be carried on for some time—for example, the effect of drugs upon bile secretion.

5089. Where there is an operation?—Where there has been an operation—in this particular case a biliary fistula.

5090. Would that be a cutting operation?—A biliary fistula in which the animal has been allowed to recover—an opening into the gall duct, for example, in order to collect the amount of bile.

5091. You have spoken very positively on the subject, but I want to ask you again, as it is a subject on which there seems to be some difference of opinion, is there no difficulty in putting a dog and keeping it for a long time during a severe operation under perfect anaesthesia?—There is no difficulty whatever. I have had a dog for six hours under anaesthesia.

5092. And without any real danger of killing the dog?—Not if the operator is careful. You can keep a dog anaesthetised for six or eight hours without the dog dying or ever recovering consciousness.

5093. And under perfect anaesthesia?—Under perfect anaesthesia.

5094. Would the same apply to a monkey?—I have absolutely no doubt. I have done it so often.

5095. Would the same apply to a monkey?—I have no experience in anaesthetising monkeys, but I think I would undertake to keep a monkey anaesthetised for as many hours as I wanted to do so.

5096. If you do not speak for monkeys, do you know whether dogs exhibit any great signs of apprehension in the laboratory?—They exhibit none at all.

5097. Not before the operation?—No. They do not suffer at all in that way.

5098. One witness rather deprecated the use of dogs and monkeys if it is avoidable, on the ground that they are animals of higher intelligence and sensibility, and seem to suffer great apprehension before the operation. You do not agree with that view?—No, I have had dogs in laboratories for months at a time—operated-upon dogs generally.

5099. And apparently showing no signs of apprehension?—None at all; they are perfectly happy so far as I can make out.

5100. Then about the American law, I suppose the ordinary law of cruelty obtains?—Yes.

5101. But there is no special regulation with regard to experiments on living animals?—No.

5102. (*Mr. Ram.*) Just following up the point about America, may any man perform any operation on any animal in any place so far as the law is concerned?—So far as he is not liable under the law against cruelty to animals.

5103. You say that you have never known that state of things to lead to any abuse?—No, I have not. One had the ordinary cases of cruelty to animals, such as one has here from overdriving horses and so on.

5104. But not from the scientific or research point of view?—I never heard of any.

5105. Do you think that that state of things is better or that it is well to have a prohibitive law such as we have here, a safeguarding law?—I do not know. American physiologists seem to think that a prohibitive law would be worse, and the anti-vivisectionists in America seem to think it would be better, because the one is striving to get the law and the other is striving to prevent it. I do not think it makes much difference.

5106. Is there a body of anti-vivisectionists in America who take a very active part in the subject?—Yes.

5107. What do they ask for?—They ask for a law, I believe.

5108. Such as we have?—Something after that sort. I do not think they have gone the length of advocating actual abolition, but I should not like to be sure about that.

5109. Do you think you could ascertain it for certain?—Yes, I could ascertain it.

5110. I should be glad if you would do so and let us have the information?—I shall be very glad to do so.

5111. (*Dr. Gaskell.*) Does the demand from the anti-vivisectionists in America come to each State?—Yes, it would have to be a State law.

5112. Therefore the demand might be different in different States?—I think so. The reason why I am a little doubtful is because there never has been any general demand.

5113. (*Mr. Ram.*) Is there any large and authorised society of anti-vivisectionists?—An authorised but more or less fundless society in the East.

5114. Could you find out what it is they ask for?—I shall be very glad to do so.

5115. You told us with regard to the experiments made upon dogs, for instance as to digitalis, in the first place that it had been used for a long time blindly, and in some instances deleteriously?—Yes.

5116. That experiments on animals then showed the true use of digitalis and the wise use of it?—Yes.

5117. And that it has been practised on that principle ever since?—Yes.

5118. Has the fact that there was in that way discovered a drug which had been used for a long time without its accurate use being known, induced experiments to be made with regard to other well-known and long-known drugs in order to see whether they are being wisely used?—Yes, every drug or almost every drug that has been widely recognised in medicine has been investigated and has had a certain number of experiments performed with it in the last 30 to 35 years.

5119. That of course is a comparatively modern safeguard?—Yes. And there are numbers of old drugs which have been resuscitated; I mean that they have had a new reputation lent them by those experiments; for example, the old drug squills.

5120. Then there are many other cases besides that of digitalis in which what I may call the improved use, the more scientific use, has been discovered?—Yes, in numerous cases.

5121. That I suppose may be still going on and will go on?—I hope it will go on very soon on other drugs now.

5122. Are they all tested upon animals now?—They are generally tested before they are put on the market by the more reputable firms, and then they have to run the gauntlet of a second series of investigations by more impartial investigators in the University Laboratories.

5123. Is it now the fact that the use of all drugs on the human body is defined and elucidated by being used on animals before being tried on the human body?—Yes, in every case now.

5124. That would give much greater safety to the use of all drugs in the human body?—Yes.

5125. With regard to these experiments on animals with drugs do any of them cause any pain in the animal?—Yes, a certain number of them. In a certain number the animal dies.

5126. Does it die from the poison produced in the drug?—Yes; if it is an active drug one has to ascertain the poisonous dose.

5127. Would there be any symptoms of pain?—Not many symptoms. The rabbit and guinea-pig really do not give many symptoms of pain; they are rather dull animals.

5128. What about the dog?—One does not use a dog for that purpose at all. One would test it first on a rabbit and a guinea-pig to find the toxic dose, and then one gets a notion whether the drug causes convulsions. If it causes convulsions, one practically never gives it to a dog or a cat. On the other hand, if it is a drug that is not very toxic, but suggests that it may be of use as a diuretic, one would try a small dose on a dog. But one never tries on an unanaesthetised dog an experiment that would cause any pain at all.

5129. If any of these experiments were calculated to give pain, and the drug were used on a dog, that would require a certificate as well as a licence?—Yes.

5130. (*Dr. Wilson.*) Not unless it was a cutting operation?—Yes, unless the dog was anaesthetised.

5131. (*Mr. Ram.*) It is not merely a question of

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cutting, because the words of Section 5 are: "An experiment calculated to give pain shall not be performed without anæsthetics on a dog or cat except on such certificate being given as in this Act mentioned," so that you will have to be sure that there would be no reasonable probability of any pain being inflicted by the experiment if you were proceeding under a licence only?—Yes.

5132. Sir Mackenzie Chalmers asked you one or two questions about the use of local anæsthetics upon animals. When a local anæsthetic, such as cocaine and the rest, is used, is there total anæsthesia, although perhaps not deep anæsthesia?—No, it is deep anæsthesia, but confined to certain localities.

5133. But you told Sir Mackenzie Chalmers that you did not think it would be safe to rely on the use of local anæsthetics in cutting operations on animals?—I think if it was a small operation, in which no observations were involved, it might perhaps be sufficient, but where actual observations were involved, the animal would be certain to be afraid.

5134. The animal would not lose consciousness, though it would lose the sensation of pain?—It would lose local sensation of pain.

5135. But it would be conscious of being held and restrained, and it might be frightened and struggle?—Yes.

5136. Not from pain, but from apprehension?—Yes.

5137. (*Dr. Gaskell.*) And besides that, most experiments require instruments of precision?—Yes.

5138. (*Mr. Ram.*) Are local anæsthetics used at all upon animals, do you know?—I have never come across any cases of operations under them. It is much more difficult to get the complete anæsthesia.

5139. You said in an early part of your evidence that much work was done on mammals by heart exposure. I take it that in everyone of those cases the animal would not only be under anæsthesia, but under anæsthesia from which it could not recover?—Yes, certainly.

5140. Then it is only the few operations, is it not, which involve the examination of secretions from ducts and so forth that demand the recovery of the animal from anæsthesia?—Yes.

5141. And they are very limited in number?—Yes, they are very limited in number, and very limited in discomfort even.

5142. (*Dr. Gaskell.*) I want just to ask you, with respect to your remark about any alteration of the Act, do I rightly gather that what you would like is that you should not be limited to the statement of the particular animal to be used under Certificate B, but that that Certificate B, when given to you, should enable you to make experiments on different kinds of animals?—It is simply a relaxation rather of administration than of the Act itself.

5143. You said that you would have to get a new certificate, because the Lister Institute had asked you to do some experiments on rats, and that you had got a certificate to do them on almost every other animal?—Yes.

5144. And therefore you would like that sort of alteration?—Yes, and a similar loosening of several points. Another example was that I had a licence to inject dogs hypodermically, and the question arose whether I could give those animals drugs by the mouth, when I had a certificate allowing me to inject; that seemed rather an extreme case requiring some alteration.

5145. And, of course, each separate application means delay?—It means a delay very often of three or four weeks.

5146. So long as that?—Yes, sometimes three or four weeks at least.

5147. You were asked about the effect of a drug on an animal not being applicable to a man, but would you not say that the nearer the animal approaches to the status of man, the more it would be applicable?—Yes. That is the reason why one is so very anxious to preserve the permission to use dogs and the higher animals.

5148. Therefore you would feel, of course, that much more reliance could be placed upon them if the experiments were on monkeys?—Yes.

5149. In the course of your evidence you have given us the results of operations upon animals for finding out the action of drugs, but you have not mentioned one animal at all, namely, man. I suppose there have been many experiments conducted on man for the purpose of investigating the activity of drugs. Can you say anything about that sort of experiment?—I think that almost every man who is going to introduce a drug, first, after investigating its action on rabbits, dogs, frogs, and so on, tries it on himself before trying it on a patient; if he finds it acts upon himself, he goes on to use it in medicine. There have been some examples of drugs being introduced not through animal experimentation, but through experiments on man, which have rather discouraged the rest of us from following that method. The one which I have given already was Christison, who ate the Calabar beans without any previous experience, and almost died of it. Another was Koeppel, who tried the effects of digitoxin on himself, and was despaired of for 24 hours; and a third case I was told of quite recently, which shows the absolute necessity for knowing something of a drug before using it, was in the case of the investigation of chloroform. I was told that Simpson, Matthews Duncan, and Keith investigated the question of a number of those remedies; chloroform turned out to be one of them. Another one which they investigated before they took up chloroform was prussic acid, and it was simply a chance that they did not inhale prussic acid, but put their hands into it instead of inhaling it. Otherwise, of course, the world would have been the poorer by the want of chloroform for an appreciable time, and also by the want of those three men. That shows the necessity which is now realised of investigating things on animals before they are investigated on man.

5150. There is one question I would like to ask you with respect to this anæsthesia in animals. We have been told again and again that morphia is not an anæsthetic, and that when dogs have had morphia given to them they may and very probably do suffer during the experiment. Have you used morphia at all?—I have used it very frequently. I generally use it along with another anæsthetic, but I have used it alone. There is no question about its anæsthetic effects, because it has recently become a rather fashionable anæsthetic in man. I was reading just the other day a series of statistics by Wood, which gave the statistics of 1,800 and some odd cases of anæsthesia by morphin and hyoscin.

5151. (*Chairman.*) Do you mean administered by a doctor?—Yes, administered by doctors recently for surgical operations in over 1,800 cases, and his only objection to the method was that it was more fatal; the statistics were worse than those under chloroform and ether. I think that out of the 1,800 and odd cases he got one death in some 220 or 250.

5152. (*Dr. Gaskell.*) Owing to an overdose?—Yes, it was owing to an overdose in man. It would have been a mere trifle in an animal.

5153. It is a fact, is it not, in the case of morphia, as in that of urethane, that the anæsthesia deepens after the drug has been administered?—The anæsthesia deepens particularly if one has a mixed anæsthesia. If you begin with morphin and give a mere trace of chloroform afterwards you can keep the animal anæsthetised for hours without any fresh chloroform.

TWELFTH DAY.

Wednesday, 27th February 1907.

PRESENT.

The Right Hon. The Viscount SELBY (*Chairman*).

Colonel The Right Hon. A. M. LOCKWOOD, C.V.O., M.P.
 Sir W. S. CHURCH, Bart., K.C.B., M.D.
 Sir W. J. COLLINS, M.P., M.D., F.R.C.S.
 Sir J. MCFADYEAN, M.B.
 Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.
 Mr. W. H. GASKELL, M.D., F.R.S.
 Mr. J. TOMKINSON, M.P.
 Mr. G. WILSON, LL.D., M.D.
 Captain C. BIGHAM, C.M.G. (*Secretary*).

Mr. A. R. CUSHNY, M.A., M.D., recalled; and further Examined.

5154. (*Dr. Wilson.*) I am right, am I not, in stating that you studied at Aberdeen University, and won what is called the Thomson Fellowship there?—Yes.

5155. Was that Fellowship allied to what is called a research scholarship in the London medical schools, or was it a Fellowship to enable you to proceed, after taking your degree, to the Continent and prosecute further study there of any kind?—It is a travelling Fellowship.

5156. Not a research scholarship?—One is expected to do research, but one has to go abroad for at least one year.

5157. Then you studied at Strasburg University?—Yes, at Berne, and then at Strasburg.

5158. And you became assistant to Professor Schmiedeberg?—Yes.

5159. During your student days at Aberdeen University, may I ask whether the lectures on pharmacology or materia medica were illustrated by experiments on living animals?—Yes, that was the advance made in pharmacology when Dr. Cash was appointed.

5160. In reference to a question put to you yesterday, I think you said that experiments on animals were as necessary for the efficient teaching of pharmacology as dissecting a dead body is in practical anatomy?—I believe so.

5161. But is there not this great difference, that a course of dissection, even in my time, was made compulsory, and these experiments on animals are not made compulsory so far as the students are concerned; they are not compelled to see them unless they choose?—They are compelled to attend demonstrations. It is part of the course.

5162. They must see the experiments on animals?—Yes.

5163. Whether it is repugnant to them or not, they must be present?—I have never met any student who was not so interested in the result as to overcome his natural dislike to blood, and that sort of thing, exactly the same as in surgery.

5164. May I ask whether it is within your knowledge that these experiments to illustrate lectures in pharmacology are carried on in the majority of the medical schools in this country?—I should not like to make a definite statement on that matter, because there are so many small medical schools in which it is not carried on; but on the other hand, there are so many larger medical schools, and at the larger medical schools I think demonstrations are carried on, as, for example, in the Scotch Universities, and in Cambridge, and in both University College and King's College here, demonstrations are carried on.

5165. But is there not one university, I think it is Nottingham, one of the new universities and new medical schools, where they say the governing body will not have animal experiments at all, either on pharmacology or physiology?—I do not think pharmacology or physiology is taught in Nottingham; I am not sure.

5166. But even in physiology they will not permit experiments in Nottingham?—I am not sure that physiology is taught. I do not think it is.

5167. However, it is immaterial. And in the Medical School for Women here in London they do not permit these experiments; is not that so?—I think that is a mistake.

5168. In teaching physiology, for example, at the Medical School for Women in London, do they permit experiments in the physiological class, do you know?—I believe so. One of the most distinguished vivisectioners in London teaches physiology there, Dr. Brodie.

5169. Are not many of the best students induced to go in for experimentation on animals for the sake of winning research scholarships?—I should not like to say that. I really am not quite conversant with how research scholarships are obtained, but I do not think vivisection is a necessary part of the course. At any rate, vivisection on mammals is not a necessary part of the course.

5170. In testing the effect of any remedy, does not it often happen that conclusions derived from experiments on healthy animals are found to be erroneous when applied to the diseased condition of human beings—that we often find errors cropping up?—I should not like to say often.

5171. But they do crop up?—I do not think you will find more errors than in drawing inferences from one human being to another.

5172. I will take for example your own experiments on the effects of drugs on the uterus as recently described in the "British Medical Journal." That paper, I think, was read at a meeting of the British Medical Association at Toronto, was it not?—Yes.

5173. Did you not use dogs, cats, and rabbits in those experiments?—Yes.

5174. It is also stated in the account in the "British Medical Journal" which I have read, that you anaesthetised these animals with morphia and paraldehyde?—Yes.

5175. Now, while I have not the slightest doubt, after your positive statements about complete anaesthesia which you made yesterday, that you honestly believe that these animals suffered no pain, I wish

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to ask you a few direct questions in order, if possible, to reassure others, who, like myself, are not altogether certain that complete anaesthesia can be maintained through such prolonged and very severe experiments with absolute certainty. May I take it that all who experiment on animals contend that it is the relatively large doses of these narcotics which are used which place complete anaesthesia beyond question? That is the position you take up, is it not?—That is the position in the main.

5176. Is it not possible that, although the animal may be so completely narcotised that it cannot struggle, the sensory centres may still be capable of receiving impressions at intervals; that, in fact, a kind of cataleptic condition may occasionally intervene?—All the evidence goes to show that the sensory side, speaking roughly, is paralysed long before the motor.

5177. That there can be no trance condition induced; that is to say, that the animals might not be able to move or cry and yet feel?—No, there is no evidence of it.

5178. Are you absolutely certain?—It is absolutely as certain as any fact can possibly be in medicine, that in anaesthesia the sensation, what is popularly known as feeling, goes before motion goes. That you can see on any operation table almost; the patient continues to move his arms or legs very often throughout the operation, though at the end he tells you that he had not the remotest idea what was going on; he was quite unconscious.

5179. With regard to these narcotics which are used—and you will pardon me for pressing you upon the point, not because I doubt your good faith at all, but because I wish to reassure, or, at any rate, make the matter clear so far as the readers of papers may be concerned—do you agree with Professor Whittle in the following extract from his well-known text book on pharmacology with regard to paraldehyd? That is one of those drugs that you use?—Yes.

5180. "It is especially valuable as a hypnotic, but, like the newer hypnotics, it has no effect where pain is present." Of course, that only applies to human beings when it is used in ordinary cases of illness?—Yes.

5181. But your contention is that when it is given to an animal the narcosis is so complete that all pain or risk of pain is banished?—The initial effect of all those narcotics is to produce sleep, which, in the presence of any strong interfering agent such as pain, is delayed. Sleep is delayed or prevented, and may be prevented entirely, by any strong excitation, such as pain, if only small doses are used, such as in man; but in the man the dose of paraldehyd, for example, is generally up to about a drachm. In a man of average weight, say 150 lbs., we use this dose, and in the rabbit of 2 lbs. or 3 lbs. we never begin with a smaller dose than is used in medicine for a man of 150 lbs. I always use, say, 2 drachms in a small rabbit, in a moderate size rabbit, whereas a physician would use one drachm in a large man. And this induces the very deepest stage—a stage that is never seen in man at all.

5182. The dose in man is to produce sleep, of course, and not anaesthesia?—Not anaesthesia at all.

5183. (Chairman.) And the dose in the rabbit is to produce anaesthesia?—Yes.

5184. (Dr. Wilson.) I am referring now to paraldehyd. Is that what you were referring to just now?—Yes.

5185. You maintained yesterday, and it has been maintained before at these sittings, that that is largely used for experiments on rabbits?—Yes.

5186. I have looked up your own excellent text book on pharmacology, and I find this statement concerning urethane: "In many cases it is almost a perfect hypnotic, producing light sleep, but in others it seems to have no hypnotic effects." You are correctly quoted?—Yes, I think so.

5187. Of course, that also applies to its use so far as the human being is concerned; but in reading, would not that alone tend to cast some doubt on its reliability as an anaesthetic to students who read your

book, that is to say, unless it was explained fully?—The reference is to its hypnotic influence. I am afraid I have not said anything about its anaesthetic influence there; its anaesthetic influence is elicited in rabbits in a dose about 100 times as great in proportion as is used as a hypnotic in man. The dose is so enormously different that there can be no comparison.

5188. It is also stated that in respect to all the animals operated upon which you described there, you opened the trachea and inserted a cannula?—Yes.

5189. May I ask why you considered that necessary? It was not for artificial respiration, was it?—It was for artificial respiration partly and partly because the animals were sunk in a bath of saline—in a warm bath.

5190. But when a cannula is inserted into the trachea would not the animal be deprived of giving any of the usual indications of pain—they could not cry?—They could not cry. They could move perfectly well.

5191. (Colonel Lockwood.) They were anaesthetised before they were put in, were they?—They were anaesthetised before the trachea was opened.

5192. (Dr. Wilson.) I have seen it stated that even the great Pasteur was very fond of animals. May I ask whether you yourself are fond of animals?—Yes.

5193. Have you a pet dog?—I have always had a pet dog since I can remember. I have had a series of them.

5194. May I therefore take it from you that whatever anaesthetic or narcotic may be used there can be no cruelty in these operations, because there is no pain?—There is no pain.

5195. And no matter how harrowing the reading of the operations may be, the reader may rest assured that no suffering has been inflicted?—In my experience no suffering has occurred as the result of the operations.

5196. And now a question or two as to the extreme difficulty of arriving at reliable conclusions in respect to many of these research experiments. And, first, is the following a correct statement of the details of the experiments on the uterus which I have just been referring to? I will just read a sentence or two: "The animals were anaesthetised with morphin and paraldehyd, a cannula was placed in the trachea, and the animal was then immersed in a bath of physiological salt solution, heated to 98 degrees Fahrenheit, and maintained at that temperature. The uterus was exposed by an incision along the median line, the hypogastric nerves were generally cut, and a system of levers resembling a myocardiograph was attached to one horn of the uterus, by means of which the contraction of the organ was transmitted to a lever writing on a slowly moving smoked surface. The blood pressure in the carotid was also recorded in the usual way, and the drugs were applied by injection into the jugular vein. A considerable number of my experiments were performed on animals in different stages of pregnancy, and, as a general rule, in these the movements set in more promptly, and were more extensive than in virgin animals." All that is correct?—I think so.

5197. Now, as to the result: "In the course of the investigation it soon appeared that diametrically opposite results were often obtained in different animals from the same drug. Thus nicotine caused marked contraction of the uterus in the virgin rabbit, while in the virgin cat the organ relaxes under it," and so on. Now, if you found that "diametrically opposite results were often obtained in different animals from the same drug," is it not reasonable to infer that no, I was going to say, legitimate conclusions can be drawn in respect to their applicability for the relief of certain emergencies in pregnant women. Is there not great room for erroneous conclusions being drawn?—I should have added that diametrically different results were obtained from the same species of animals also; that, for example, a pregnant cat reacts differently from a non-pregnant cat, and that, of course, it is very possible that a non-pregnant woman reacts differently from a pregnant one in regard to the uterus. But it is an important fact to show that that can occur—to suggest the idea that a pregnant uterus reacts differently from a non-pregnant uterus.

5198. Is there not also great liability to error arising from the entirely different conditions and circumstances of an animal lying with opened abdomen in a saline bath, and a woman in labour, or a woman who is not in labour?—We tried to approximate the conditions as far as possible. The saline bath is supposed to represent the closed peritoneal cavity. We cannot keep the cavity closed and observe the uterus, but we protect the uterus from external agencies as far as possible in the same way as a woman's uterus is protected from external agencies. That is the object of the saline bath.

5199. But the object of the saline bath I always understood was to keep up the temperature?—No, the main object is to keep the uterus surrounded with a non-injurious fluid; that is the main object.

5200. Then the animal also would be suffering from all the disturbing factors of severe surgical shock, would it not? These are very disturbing factors?—That has to be taken into account. The animal is suffering from severe surgical shock and other factors before the drug is applied. The same factors remain; but you see the difference which is obviously due to the drug.

5201. And you experimented, did you not, with a long list of drugs, such as nicotin, adrenalin, atropin, pilocarpin, quinine hydrochlorid, aloes, and ergot, as well as others?—Yes.

5202. Has not ergot been used as a drug for women in labour long before these researches on animals were entered upon?—Yes.

5203. Now frankly do you think that either Dale's experiments to which you refer, or yours have really thrown any new light upon the use of ergot, which clinical experience has alone made useful and applicable in midwifery practice?—Clinical experience with regard to ergot is exceedingly divided, as you are probably aware.

5204. Yes?—Half the clinicians maintain that ergot ought to be given early in the course of labour; the other half maintain that it should be given late in labour, only when the child is expelled. The two schools are at opposite poles as regards practice; and any light that experimental work can throw upon it is undoubtedly going to be of value, simply in determining which school is the correct one. Then a further point to be got out—a further point which I was anxious to make—is what is the sure test of the action of ergot; because there are so many ergots on the market, half of them inactive, and a lot of others of doubtful activity—that I wanted to work out exactly what is a test result that one could use and say, "This is the ergot effect. When the next patent form of ergot comes up let us try this, and find whether it has really the ergot action."

5205. But at the same meeting did not Dr. Dixon, of Cambridge, narrate a good many experiments which he had performed with regard to testing ergot on animals?—Yes, I believe he did.

5206. And his test was the blood pressure, was it not?—Yes, I think so.

5207. And according to his paper his results were also very varied, to use his own expression, that is to say, that they were not at all constant as regards the blood pressure—that it varied very much in his experiments?—From ergot preparations of different sources you mean?

5208. Yes?—That is because much of the ergot in the market is of very inferior value. The chemist cannot control that. It has to be done by experimental work. One preparation at present in the market is quite useless.

5209. Is it not possible that these variations may depend as much upon the particular animal as upon the preparation?—One has to take that into account. The best way of doing that is to test one preparation of ergot against another on the same animal, which can be done.

5210. Now special reference was made yesterday to the action of digitalis on the heart, to take another drug. Do not you think a good many of these drugs have a composite action, that is to say that they may

act upon more than one function of the body; that digitalis for instance may be a diuretic as well as a heart stimulant?—Yes, digitalis acts upon a number of different functions of the body.

5211. And that would apply to a good many other drugs on which experiments have been carried out?—Yes, a large number of them, ergot notably, the drug we have been talking of.

5212. So that in taking merely the blood pressure say as the index, or the contraction of the uterus as the index, your whole experiments can be said by no means to be complete to enable you to draw a definite conclusion?—It is exactly the same point as the chemist has. A chemist in estimating the presence of a certain amount of, say, an acid, takes a certain alkali—he takes, say, sodium hydrate; he might take potassium hydrate; but he is quite satisfied with sodium hydrate in ascertaining the strength of his acid solution.

5213. But you see in a chemical laboratory you are dealing with inert matter—I mean inert dead matter, not living matter. That is a great difference, is it not?—You are dealing with a much simpler reaction than we are dealing with in a pharmacological laboratory.

5214. I think you also laid special stress upon experiments on animals with regard to digitalis in educating medical men how to treat pneumonia; that is to say that in the old days the treatment in pneumonia was rather erroneous, so far as the use of digitalis was concerned?—Yes. It was at one time erroneous.

5215. But surely in this country the treatment of pneumonia by digitalis was never regarded as a routine treatment, was it?—Not in this country I think so much as abroad. This country has very often been ahead of the rest of the world in therapeutics; but if you take the general course of therapeutics at one time digitalis was distinctly abused.

5216. On the Continent?—On the Continent more particularly. It was on the Continent that these experiments were carried out really.

5217. But in those early days they regarded tartar emetic as the popular remedy?—Yes.

5218. And then in my time, and subsequently in yours, the expectant treatment was so regarded?—Yes.

5219. That did not last very long?—No.

5220. Then in recent days, of course, when bacteriology came to the front the serum treatment was believed to be the only scientific treatment of pneumonia. There were a great many who contended that, were there not?—I should not say a great many, I think.

5221. Do you think there is great doubt about it now?—I should not like to express an opinion of the toxin treatment, it is rather out of my line.

5222. Yesterday you laid special insistence upon the value of experiments on animals as to the discovery and therapeutic use of chloral?—Yes.

5223. Is that as popular a remedy now as it used to be?—I put it in this way, that 35 years ago that was the only soporific; now that there are other soporifics, of course it is not as universal as it was immediately after its introduction.

5224. But do you think there is any need for all this constantly increasing crowd of hypnotic and soporific remedies?—I do not think we have reached an ideal soporific yet. I think we must go on until we find one.

5225. I saw in your *précis* that you did not make any reference to alcohol. Have not experiments been carried on with regard to alcohol as largely almost as with respect to any other drug?—A very large number of experiments have been made.

5226. And there is still some wide divergence of opinion?—Not among scientific men, I think.

5227. But among medical men, are you not still puzzled as to the dose and the medium—whether spirits or wine?—I was referring particularly to the experiments carried on in regard to its food value.

5228. I am referring to its medical value, not Att-

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water's experiments and Parke's. I am referring to its action as a drug?—Yes, a certain number of experiments, not a very large number, we will say. There has been, of course, the further question debated a good deal as to its effect on the circulation, and that has required a good deal of elucidation by experiment, and a considerable number of experiments have been done on that subject.

5229. Now with regard to almost all those drugs to which you have referred in your *précis*, and which we have more or less discussed, are they not comparatively new drugs that have been experimented with?—They have all been introduced in the last 35 years.

5230. But is it not the fact that the great majority of them can only at the best be regarded as palliatives, and not as curative medicines, that is to say you cannot say that they will cure disease?—There are very few drugs found in any way that actually cure disease. If one excludes mercury and quinine and, perhaps salicylic acid in acute rheumatism, I think there are very few others that one can say are actual cures for disease or specifics.

5231. Is it not also the fact that the great majority of them are classed as unofficial remedies; I mean that they do not appear in the British Pharmacopœia?—That is quite true, because it is ten years since the last edition of the British Pharmacopœia. A considerable number of them have been incorporated in the new United States Pharmacopœia, which came out only the year before last.

5232. (Colonel Lockwood.) Do we recognise that?—No, but the British Pharmacopœia only comes out, you see, once in 10 years. So does the American. The American, of course, gets ahead of us.

5233. (Dr. Wilson.) Would you contend, then, that the drugs which are described in the British Pharmacopœia in hundreds of thousands, I may say, are not a sufficient equipment for any medical man to practise his profession successfully?—I think not. I think the profession would feel the loss of chloral and its fellows.

5234. Chloral is in the British Pharmacopœia?—Yes, it was put into the last edition. I think that if the Pharmacopœia does not admit adrenalin the physicians will continue to use adrenalin, even though the Pharmacopœia does not admit it, and so on.

5235. In the British Medical Pharmacopœia are there not recognised official tests for drugs set down?—For most drugs.

5236. There are Acts in this country called the Food and Drugs Adulteration Acts?—Yes.

5237. They are intended, of course, to maintain the purity of drugs as well as the wholesomeness of foods?—Yes.

5238. Do you think they make it incumbent upon public analysts to experiment on animals as to their purity or efficiency?—I think if a public analyst pretends to state that any specimen of ergot is useful, he is bound to carry out experiments. But as a matter of fact, I believe that if a public analyst were asked whether a preparation of ergot was pure, was unadulterated, he would be able simply to state that it accorded with the description in the Pharmacopœia and escape all further legal question, although, of course, he is quite unable to state that it is a useful specimen.

5239. Do you know anything about the Greifswald University in Germany?—I know of it.

5240. Do you know Professor Schulz, who attended the opening ceremonial on the four hundredth anniversary, I think it was?—Yes, I have heard of Professor Schulz.

5241. I have here a report of a paper by him, which is as follows:—"He chooses the subject of the testing of medicaments on healthy subjects for his theme. Opinions as to the value of this form of experiment vary very much, but Schulz believes that if every pharmacist who brings out a new drug were first of all to try the effect in full doses on his own body the number of more or less useless preparations would be materially lessened." Do you agree with that?—The number of pharmacists also, I think, would be materially lessened.

5242. He also maintained that "The difference

between a healthy animal and a sick man is very great, and one is, therefore, not justified in leaving the healthy man out of the series of experimental tests." Do you agree with that?—I think Schulz for the last 40 years has advocated the testing of drugs upon healthy men, notably on his students.

5243. I was going to ask you about that. Do you get volunteers among your enthusiastic students to assist you at all?—I have had a certain number of experiments done on students by their own desire.

5244. But supposing that you settled that a drug is not poisonous (you would not give him a poisonous drug), would not it be a much more efficient way of testing the properties of drugs than on animals to get a volunteer experiment regiment, as it were?—Then you cannot analyse the effect of the drug at all.

5245. Yes, you could with regard to all these soporifics, and so on?—Yes, you can make out a soporific; but whether the health was badly affected by any soporific or not, you would hardly be able to make out very well. A healthy student's heart would stand a good deal of knocking about that a sick patient's heart will not. The heart of a student might not show the slightest material change in his pulse, and you would not know whether there is a fall in blood pressure. You cannot do that in a student, you have to do that on an animal. I have tried several drugs upon myself in considerable doses, not of course in dangerous doses, and students have also done the same.

5246. Have not American and Continental manufacturing chemists led the way in the manufacture of many of these new drugs—most of them, I may say?—German chemical firms, of course, have introduced an enormous number of drugs.

5247. And from America too?—A certain number of new drugs, yes—not so many.

5248. Is it not also true that a large proportion of the people in America have either discarded all belief in doctors, and become Christian Scientists, or Faith Healers, or have become drug swallows without consulting a doctor at all?—I should not like to give the proportions of the population which belong to either camp. Of course there is a large amount of unauthorised drug-taking in America, and there is also a considerable camp of Christian Scientists, although not, perhaps, so many as one is led to believe occasionally in this country.

5249. Is it not the competition of these manufacturing chemists which is flooding the market or the country with all these new remedies?—I think there is no question that a large number of the new remedies are no advance upon those formerly used. I think that is quite correct; but it is quite legitimate to introduce a new drug in the hope of its being an improvement. Whether it is an improvement ought to be ascertained by careful examination before it is thrust upon the market; and those improvements can best be made out by animal experimentation, I maintain, and careful comparison.

5250. And in testing drugs on animals, so long, of course, as there is no cutting or injection, no licence is really required by the Vivisection Acts. So long as you do not inject or operate upon an animal you do not require a licence?—I was informed that for giving a drug by the mouth, if you wished to find out what it did, if it was an experiment, you required a licence, and also a certificate.

5251. (Colonel Lockwood.) On a man?—On an animal. I believe the law is that if it is a test, if you do not know what is going to happen, that it is an experiment. If you know what is going to happen, as in injecting toxin or in drawing antitoxin, then you do not require a licence.

5252. (Dr. Wilson.) At any rate, may I conclude, then, that so long as these new drugs are being constantly manufactured so long must you go on, or must experts go on, testing them?—Exactly.

5253. As much to prevent people being poisoned as to give them the chance of cure?—Yes, very much so.

5254. (Chairman.) You told us that dogs are kept for five or six hours under complete anaesthesia?—Yes.

5255. What is the longest time that human beings can be kept under continuous anaesthesia?—I do not know that there is any particular limit.

5256. What is the longest time they have been kept, for some operations require a long time?—I really should not like to give a definite answer.

5257. (*Colonel Lockwood.*) A couple of hours?—More than a couple of hours. They have been kept four hours, at any rate, under anaesthesia.

5258. (*Chairman.*) But the experiment of testing them to breaking point has never been tried?—No.

5259. (*Sir William Church.*) You have been asked a great many questions upon the curative nature of drugs, but are any drugs curative of disease, or can they be said to be an antidote to disease?—I specified three that I would be prepared to call specifics—quinine for malaria, and mercury for syphilis, and possibly salicylic acid or salicylates in acute rheumatism, although it is scarcely in the same plane of certainty as quinine.

5260. Perhaps you will explain to the Commission, as you call those specifics, how quinine acts with malaria?—It simply kills the cause of malaria.

5261. You would hardly regard that as a specific for disease, rather as a specific against the plasmodium?—Yes.

5262. And mercury for syphilis in the same way?—Very probably.

5263. But the object of pharmacology is not to find out curative means directly, I mean antidotal bodies. No pharmacologist would expect to discover a body which will cure pneumonia?—No. The main object, of course, which is not brought out, is that the pharmacologist learns how organs are to be influenced; and, as a general rule, in disease, in therapeutics it is not diseased organs that are acted upon, but sound organs, which one wishes to substitute for diseased organs.

5264. The whole object of pharmacology is to find out the way in which different drugs act upon the human economy?—Yes.

5265. And, taken with the study of disease, our object is to get hold of drugs which, while assisting the other natural operations of the body, at the same time do not do any harm?—Do not do any harm—leave the body in a sound state, so that it can cure itself.

5266. Then the only way in which we can discover the result of the action of any drug is by watching and making observations upon its action on different portions of the body?—Exactly.

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5267. And just to turn for one moment to these new substances, you have been asked a great deal about soporifics; none of them can be in any way said to be curative, can they?—No; they aid the body in curing itself, I should say. That is to say, they rest the brain, for example, and prevent movement, and that puts the organism in a state in which it is more likely to recover.

5268. Therefore the great object is to discover a drug which, whilst it causes a loss of activity in some portion of our economy, does not do any harm in another part?—Exactly. That is what I mean by saying that the ideal is not yet attained.

5269. So that it is quite a mistaken idea of pharmacology to think that it is searching for antidotal substances?—Yes, for direct antidotes to disease.

5270. (*Sir Mackenzie Chalmers.*) Where is the line of demarcation drawn between anaesthetics and analgesics—they both obliterate pain?—The line is purely artificial. There is not a line in nature. It is simply a matter of dose. A small dose of chloral or of morphin is an analgesic, or a hypnotic, or a soporific. A large dose of the same drug is an anaesthetic. A small dose of ether, for example, may also be looked upon as a soporific.

5271. I asked you the question, because you gave us one set of substances which you referred to as anaesthetics, and another set of substances which you referred to as analgesics?—That is one of the faults of being an author—knowledge is put in compartments.

5272. (*Chairman.*) Is the word analgesic usually applied to outward application?—No.

5273. (*Sir John McFadyean.*) Is it the same distinction as between a laxative and a purgative?—Yes, it is exactly the same thing. One is a more powerful drug than another.

5274. It is a question of dose?—Yes.

MISS ARABELLA KENEALY, L.R.C.P. (Dublin), called in; and Examined.

5275. (*Chairman.*) You are, I believe, a licentiate of the Royal College of Physicians, Dublin?—Yes.

5276. And you have, I understand, practised for a time in London and at Watford?—Yes, for about eight or nine years.

5277. When did you cease to practise?—About seven or eight years ago.

5278. Since then, I think, you have been engaged in literature?—Yes, wholly.

5279. And you have taken great interest in this question of experiments on animals?—For the last two or three years only.

5280. You have sent us a short *précis* of the subjects upon which you wish to give evidence. Would you take them in your own order, please, beginning with the first one, unless there is anything preliminary you wish to say?—It is my privilege to appear to-day before that which I believe to be one of the two very highest tribunals ever constituted. There have been tribunals which have tried kings, which have tried king-makers, and peers and men. All these were concerned, directly or indirectly, with men's interests, material or moral. But the Commission which you represent to-day stands high above all personal considerations, above self interest, above self benefit. It expresses the very finest and most beautiful bit of generous altruism possible to the heart of man. For the poor plaintiffs who appear before you are but frogs and ownerless dogs, the common rabbits of the field, and horses that have spent their value in the service of their masters. And yet, in the course of evolutionary progress, so just and merciful has the heart of man become that it has come to recognise the humble, but indubitable claims which these poor creatures have upon us. To all the charges of decadence, of selfishness, of greed, and of devotion to pleasure, which are brought against us, the existence of this

Commission gives the lie. If decadence is in our midst, so also evolution is. A great Government has appointed, a great people has sanctioned, and ten men and true, of standing and mental attainment, have been found to devote their services, for a whole year it may be, to investigating the case and to righting the wrongs of these poor plaintiffs. Within the conventions and machinery of these proceedings is a shining heart, the heart of a justice, a manly consideration, a human generosity perhaps unparalleled. It is better a thousand times, I think, to put out men's lives than it is to put out these shining lights by which their souls are led. I repeat that it is my privilege to appear to-day before that which I believe to be one of the two very highest tribunals ever appointed by man. Our Chairman, Mrs. Cook, has given you a few examples of the cruelties, of the published cruelties that is, which take place in vivisections. Dr. Herbert Snow, a master of his subject, has shown you the futilities and profitlessness of these investigations as regards his most important speciality, cancer. I beg leave to continue this chapter of the scientific worthlessness of vivisection. I have not the facile talents of Professor Starling. Although I am a writer of fiction, I cannot hope to persuade the Commissioners of things so rosiely improbable as that a surgical ward is a place in which persons are hilariously free from pain, that dogs come up cheerfully wagging their tails, poor things! for the privilege of being vivisected, that to be starved is a pleasing succession of lotus-eating sensations, and that the laboratory boy is so brilliant an anaesthetist that, although he errs always on the side of giving too much anaesthetic, he may yet be trusted over long periods of hours never to give the too much which would put a sudden stop to that which his master might regard as a unique and valuable experiment. Professor Starling has told you that because some physiologists and one or two professional fasting men have found ways of fasting

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which apparently caused them no inconvenience, dogs and other creatures do not suffer from being starved. I remember when I was a student, seeing in hospital a man who, too, gave exhibitions in public. He could at will, and without pain or inconvenience, disarticulate every joint in his body. That, however, does not disprove the fact that to dislocate a joint is for the normal person a very agonising experience. We have heard, too, of shipwrecked men, who, rather than endure the pains and horrors of starvation, have murdered and have eaten their comrades. No other sort of suffering, I think, has such a record. I do not, therefore, think that, apart from laboratory conclusions, we are justified in withholding our sympathy from those poor creatures, man or dog, who are subjected to the tortures of starvation. As to Professor Starling's other optimistic pictures, his glowing accounts of the marvellous discoveries in physiology and medicine which have been achieved by vivisection, it will be my ungrateful task to show you that, apart from the truths which anatomy, histology, and pathology have taught us, we are still in states of profoundest ignorance and confusion as to all but elementary facts. To use his own words about diabetes, of which he spoke to you so glowingly the other day. In Starling's Text Book of Physiology, dated 1900, he says: "Although the researches into its (diabetes) causation have thrown some light on the normal carbohydrate metabolism of the body" (in his evidence before this Commission, Professor Starling admitted that personal observations made by scientists and students upon themselves had done much toward the understanding of this metabolism) "the light is at present only sufficient to intensify the black shadows of ignorance that obscure almost every part of our knowledge of the subject." If he had pushed home his metaphor, he would, I think, have seen that these intensified "black shadows" have resulted from the fact that the light has been thrown mainly from a wrong angle. True knowledge never obscures, but, on the contrary, opens up avenues to still further knowledge. When you are lost in a wood and come upon a path, I think you will find that it does not intensify your ignorance of the way out. In the 4th Edition of Starling's Physiology, I find, on page 500, under the heading "The Ductless Glands," "Under this title has been grouped a number of organs the sole resemblance between which lies in the fact that we know very little about them." But Professor Starling apparently forgets another very close resemblance, viz., that for centuries these glands have been the subjects of innumerable vivisections. Let me show you with what result. With regard to the functions of the spleen Starling says: "The structure of this organ suggests that the splenic cells must exercise a constant influence on the blood which surrounds them, and that this influence is not purely of a chemical character." All, therefore, that is known of the functions of the spleen was suggested by the structure. And structure is revealed, post-mortem, by the eye and by the microscope. All the experiments on living animals have resulted in not a single contribution to the elucidation of the problem. Chemical analysis is referred to, as showing a number of "Extractives," which are found in the spleen. Then, it is remarked that where the spleen is enlarged by disease the excretion of uric acid is largely increased, so pointing to a connection between the spleen and the formation of uric acid. But this was, of course, a clinical observation. In Kirkes's Physiology are mentioned rhythmical contractions and dilatations of the spleen as obtained by the oncometer (an appliance for enclosing the organ of a living creature). But as Starling does not even allude to these, I take it that they have since been discredited. At all events, the nature of the oncometer might explain any sort of reaction to it on the part of a living organ. Of the function of the thymus, Starling says: "We know nothing." "But," he observes, "from the thymus a body can be extracted which, injected into the veins of an animal, causes intravascular clotting." This, of course, has been the subject of speculation and keen controversy. But Starling dismisses all these by saying: "This fact, however, does not throw light on the normal functions of the gland, since a similar body may be extracted from almost any organ that is rich in cells." Of the thyroid he says: "It was probably at one time in the history of the race a secreting gland in connection with the alimentary canal." "It is still of the utmost importance in the metabolism of the body." But this importance (of which nobody even professes to know the nature) has been shown by the grave constitutional changes which occur when it is dis-

eased. The experiments on animals have only rendered confusion worse confounded, as I shall try to show you later. Of the parathyroids he says: "Their functions are unknown." Of the pituitary body, "the functions of this body are unknown." Of the suprarenal capsules, "the functions of these glands are quite unknown, but they are supposed to influence the nutrition of the body." Starling says further of them, "As upholding the functions of these bodies in influencing nutrition through the nervous system, wide-spread degenerations in the central nervous system were described after their extirpation in animals. Subsequent investigations, however, have failed to confirm these results." Of the little we know of them, he further says, "Here again, pathology has taught us more than physiological research." Kirkes says, "The immense importance of the suprarenal bodies was first indicated by Addison, who, in 1855, pointed out that the disease, known by his name, was associated with pathological alterations of these glands." Of the coecygeal and carotid glands, Kirkes says, "The functions of these bodies are unknown." Yet all these glands, some of them large and important glands of the body, of the functions of which we know nothing, have been, perhaps from time immemorial, explored and examined by physiologists by means of countless vivisections. May we not take it, not that their functions are unknowable, but that the means used to discover them have been means incapable of apprehending them? May we not take it that their function is some subtler thing than can be detected by the crude methods of experimental physiology? For our ignorance is almost equally profound, not only with regard to these ductless glands which, because they do not form a visible secretion (found equally well post-mortem as during life), are hopeless blanks in our lottery of knowledge, but also of all those other organs which do form such secretions. Kirkes says, "Many of the glands which possess ducts and form an external secretion, form an internal secretion as well. Amongst these, the liver, pancreas, and kidney may be mentioned." The term "internal secretion" is one upon which physiologists fall back when, after innumerable experiments on animals, they have failed to obtain any knowledge of the uses and functions of the organs investigated. Starling thus candidly confesses, "We do not arrive at any nearer a solution of the question by saying that the pancreas has an 'internal secretion,' since we have no conception how such a secretion may work." Experimental physiology has been for centuries, perhaps, at work upon the kidneys. Some of the more recent cruel experiments upon animals have been described to you. Yet the profundity of our ignorance with regard to any but the obvious renal function was expressed during the recent proceedings of the British Medical Association at Toronto. In a discussion on "The Treatment of Uremia," Dr. S. Salis Cohen said: "It must not be forgotten that uremia is a phenomenon of pathologic kidneys. Removal or destruction of normal kidneys need not therefore produce it; nevertheless, they produce death by loss of kidney function. Therefore we ask the physiologists what is the kidney function? Evidently it is complex. Our gain in knowledge has enlarged the vista of our ignorance." Another backhanded blow at knowledge! But what sort of "knowledge" is this which increases ignorance and intensifies its black shadows? Would it not more properly be called error? The truth is, that the term "internal secretion" stands for that higher and intrinsic function performed by all organs—a function which is beyond the reach of experimental physiology, and which is, I think it cannot be doubted, to be found in the realm of psychology. There is not an organ in the body which cannot be said to form an "internal secretion," if by that is understood that it plays some most important part which experimental physiology has failed to discover or explain. On page 313 of Kirkes's "Physiology," in a preface to "The Ductless Glands," is the following: "The body is a complex machine, each part of the machine has its own work to do, but must work harmoniously with other parts. Just as a watch will stop if any of its numerous wheels get broken, so the metabolic cycle will become disarranged, or cease altogether, if any of the links in the chain break down." Here, now, is an intelligent conception of the working of the body! And yet experimental physiologists have not learned that the very principle of vivisection is to break one of these wheels of the watch,

one of these links of the chain, so that their observations are made only when the watch has stopped—when the metabolic cycle has become disarranged, or has ceased altogether.

5281. I have not interrupted you hitherto, but you are following a course which is quite different from that usually followed by witnesses who are heard by a Royal Commission. You are rather addressing yourself to giving us an exposition of your views generally on physiology. Our Commission is to inquire into the question whether the present Act regulating the experimentation on animals requires amending. We are not inquiring as to whether physiology is a useful science (you rather seem to suggest that it is not a useful science, so far as I gather), or as to whether the method of inquiry into it by experiments on animals is one that is futile altogether, and leads to no results. We have to inquire as to whether experiments on living animals are carried out with unnecessary cruelty and inhumanity, and those are the points to which your *précis* is mostly directed; but you are now rather entering into a general discussion as to the structure of mankind, the secrets of the human body, and the usefulness of physiology as a science. Those are matters that we should read about rather than call witnesses to give a general discussion, because they are somewhat beyond the scope of our inquiry?—My intention was to prove that it is experimental physiology which has led to all the errors and all the ignorance, and that if we were to employ the subjective method and were to examine patients and learn our physiology in that way, we should know very much more about it. At present, we are quite in the dark, really, and are ignorant as to all these organs and the functions of these organs.

5282. But I am afraid we must adopt the usual method of question and answer to elicit the information which we think is material to our inquiry. We do not wish to exclude anything that is material upon that point, but if we were to allow you to follow this course, we should have every witness saying a great deal that was not relevant to this inquiry, and it would necessarily follow that, to a large extent, we should be hearing an essay which would be a repetition of the evidence of previous witnesses on one side or the other?—I thought I might be allowed to show the futility of all these experiments.

5283. Then if you would come to that first paragraph in your *précis*, in which you speak of these experiments as being an absolute failure, we should be glad to hear what you have to say upon that point?—Then I will go on to the brain.

5284. Yes, if you deal with some definite point which you say ought to satisfy us that these experiments are useless and unsatisfactory?—As I am not an experimental physiologist, and have therefore no results of my own to offer you with which to further confound the confusions of cerebral science, I propose to offer you a few of the results and confusions of experimental physiologists themselves, taken from their own works. Those who have no knowledge of vivisection, in practice or in theory, might suppose it to be the simplest thing in the world to open a living body, to watch the living organs at work, and to describe straightway the functions and the working of these living organs as they see them. The trouble is that they do not see them. You can no more see function by looking at a living organ than you can see the passage of an electric current by looking at a telegraph wire. You may see the results of its secretion in its duct, but you can see this equally well in the dead body. Had it been otherwise, a few years of the practice of vivisection would have settled, once for all, all the problems which are baffling us, whereas the truth is that centuries of vivisection have plunged us into misconceptions and errors, which have merely obscured and retarded the progress of those other progressive and enlightened methods which have proceeded side by side with it, and to which alone is to be credited such little knowledge as we possess. If physiologists demand living tissues for their work, they need only to make use of the fact that all tissues, muscles, glands, even the brain, retain their living properties for some time after death. Moreover, they can be revived. "All we have to do after their removal from the body, however," Professor von Poehl says in his "Rational Organo-Therapy," "is to establish an artificial circulation of the blood in them with warmed defibrinated blood, and the liver will again

secrete gall, and produce urea, while the kidneys, too, will secrete. This may go on for hours. Besides, the kidneys continue their synthetical activity by producing hippuric acid out of glycocholl and benzoic acid." He cites the case of the heart of a child which 20 hours after death was so revived; and which two days after death gave muscular contractions. And, as is well known, the heart of a frog will continue to beat for hours after removal from the body, and if revived will do so for days.

5285. Will you take any case, as other witnesses speaking from the same point of view as yourself have done, in which experiments have been made which in your opinion have proved failures. I understand that you specially refer to the failure of making satisfactory discoveries about the ductless glands, or of the brain?—I have already dealt with the ductless glands.

5286. I am afraid I did not gather that you had dealt with any particular experiments?—No, not with any particular experiments. But although we have been for centuries experimenting, still we have no notion of what the ductless glands are for, which I think is a very important point. The practice is for a number of observers to make experiments, and to come to certain conclusions. Then for a later set of observers to make further experiments, and to disprove absolutely the conclusions of the first. Next come a third set of observers, who also make experiments, and divide opinion pretty fairly perhaps between the first and the second set of experimenters. Then, when you have these three sets of observers, all perhaps men with equal claim to consideration, each, in addition to denying the facts and the theories of the others, offering different facts and theories and explanations of his own, and different theories and explanations of those he regards as the erroneous facts and theories of his opponents, and, added to these, everyday observation and clinical experience frequently controverting the facts and theories and explanations of all sets of observers, you will realise that vivisection has done more to hamper and to obscure the sciences of physiology and of medicine than has any other thing.

5287. Are you now dealing with the second head in your *précis*—a criticism of certain experiments on animals?—No, I have not come to that.

5288. We really would like to come to some precise facts. We shall have to draw the inferences from the facts. There is a good deal of contradiction, as you well know, on this point?—According to Kirkes, Alkmaon as early as 580 B.C. placed the seat of consciousness in the brain. Kirkes comments, "But this was of the nature of a guess," meaning that it was not a result of experiment on living animals. So that we owe the very greatest physiological discovery on record, the discovery of the seat of consciousness, not to vivisection nor to any of its methods, but to the intuitive intelligence of a man, who in our day would, no doubt, be called a benighted heathen. Again, Kirkes says ("Kirkes's Handbook of Physiology")—"The discovery of the speech centre" (the spot in the brain which is supposed to control the power of speech) "was the earliest feat in the direction of brain localisation. It was discovered by a French physician named Broca. He noticed that patients who died after hæmorrhage in the brain, and who, previous to death, exhibited a curious disorder of speech called *aphasia*, were found after death to have the seat of the hæmorrhage in this convolution. The convolution is generally called Broca's Convolution." Kirkes adds, "Experiments on animals are obviously useless in discovering the centre for speech." The supreme importance of the cell, the growth of the body from cells, and the fact that cells are the living units of the organism, were first established in the vegetable world by Schleiden. So that the three most important physiological discoveries ever made—one, indeed, the absolute basis of the science—were made without experiment on animals. And if a science could be born, surely it could be nurtured and bred without experiments on animals! And this is what reason would lead us to expect. Not the knife and the cautery and the mutilated brains of apes and dogs, but the mind of man, with its subtle powers of intuition, of observation and deduction, aided by careful dissection, by microscopic examination, and by the revelations of pathology, is the true implement for investigating the brain of man. Intuition is the eye of the mind. By means of it only can mind be seen. Now as to the facts. Take Fishes. In his book "Cerebral Localisation," Professor Ferrier

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tells us it had generally been said (as a result of experiments on fishes, of course) that fishes deprived of their brains were like other fishes, except that they lacked spontaneity. Vulpian ("Système Nerveux," p. 669) and Steiner ("Die Functionen des Centralnervensystems; zweite Abtheilung. Die Fische." 1888), experimenting on fishes, found that when deprived of their brains, they did not lack spontaneity. With regard to frogs, Goltz ("Functionen der Nerven-centren des Frosches," 1869), and Steiner ("Physiologie des Froschirns," 1885), showed that frogs deprived of their brains behave like normal frogs, except that all spontaneity appears to be annihilated. Ferrier says:—"The more recent experiments, however of Schrader ("Physiologie des Froschgehirns. Pflüger's Archiv. für Physiologie." Band 41) would seem to show that the removal of the hemispheres deprives the frog neither of spontaneity, nor of special instincts, nor of the ability to feed itself." As to pigeons, Flourens ("Système Nerveux," 1842) showed by experiments on pigeons, that pigeons deprived of their brains were blind, deaf, and devoid of smell, taste and tactile sensibility. Magendie, Bouilland, Cuvier, and in particular Longet ("Anatomie et Physiologie du Système Nerveux," 1842) and Vulpian (*op. cit.*) showed by experiments on pigeons that, when deprived of their brains they could see and hear and feel. Munk then made a considerable number of experiments, and found that, after removing the brains of pigeons, they were totally blind. Schrader ("Physiologie des Vogelgehirns," Pflüger's Archiv. Band 44) then found that pigeons so treated could see, and avoided obstacles in their way, and fled from one place to another, and alighted securely. Also that they heard loud sounds. What does the Commission think? For my part I am at a loss what to think. Then with regard to rabbits. Whether after removal of the cerebral hemispheres, rabbits and other rodents can see, Ferrier says, in his "Brain Localisation," "is a question which has been the subject of lively controversy between Christiani and Munk ("Physiologie des Gehirns," 1885). The question is one which cannot be said to be definitely settled." As to dogs and the other higher animals, it has been found impossible to get up a "lively controversy" upon the question whether they could run, and eat, and see, and hear, after complete removal of the hemispheres, as the operation kills them. With respect to monkeys: Ferrier states that the occipito-angular region is the region of sight in monkeys. He then goes on to say that bilateral destruction of this region causes complete and enduring blindness of both eyes. He gives detailed particulars. He adds:—"There is, however, scarcely a particular of the above general statement which has not been controverted." From previous experiments, he had concluded that the angular gyri were the centres of sight. His later experiments, however, convinced him that the blindness after destruction of these was only temporary. After a time sight was perfectly regained. Then Munk ("Ueber die Functionen der Grosshirnrinde," 1881) from experiments on animals, decided that the sphere of sight was in the occipital lobe. But Ferrier attributes this to the "secondary inflammation and damage which generally, if not always, followed Munk's operations." "Munk's experiments," he says, "cannot be relied upon when it is a question of the exact delimitation of any given area." On the other hand, Munk in his book "Functions of the Brain" speaks of Ferrier's certainty in his own results as being only equalled by the impossibility of the slightest faith being placed in any of the results by anyone who examines his researches. And Ludwig, a physiologist with a great laboratory at Leipzig, compares Ferrier's experiments to injuries to a watch by means of a pistol shot (Hermann's "Human Physiology," translated by Gamgee). Luciani and Tamburini ("Sui Centri Psico-Sensori Corticali," 1879), Luciani ("On the Sensorial Localisations in the Cortex Cerebri, Brain," July, 1884), Horsley and Schäfer ("A Record of Experiments upon Cerebral Functions," Phil. Trans., Vol. clxxix., 1888, B. 20), Sanger-Brown and Schäfer ("Functions of Occipital and Temporal Lobes of Monkey's Brain," Phil. Trans., Vol. clxxix., 1888, B. 30), and others all made experiments on this question of the centre of sight, but could come to no agreement. Then Horsley and Schäfer made more extensive injuries than before, and decided that Munk's and Ferrier's and Yeo's results might be linked together as both having truth in them. Then Schäfer made further investigations with Sanger-Brown, and decided that only Munk was

right, and that Ferrier and Yeo were wrong; that the visual centre was in the occipital lobe, and that destruction of this caused blindness. Lannegrace, however, found with Ferrier and Yeo that destruction of the occipital lobe caused no appreciable impairment of vision. Then Schäfer destroyed the angular gyrus, which Ferrier believed to be the centre of sight, and found that although the monkey was blind for a few days it gradually recovered vision. And so the controversy waged. On clinical grounds, the only grounds worth consideration, Sir William Gowers regards the angular gyrus as the higher psychical centre for vision. In his Fourth Edition, however, Kirkes says, "but experimentally we know much more about the relationship of the occipital lobes to vision," showing that experiment and clinical fact are still opposed. And then with regard to the auditory centre. The same endless controversy rages about the sense of hearing. Ferrier places it in the temporo-sphenoidal convolution. When he destroyed this he found that it caused absolute deafness. On the other hand Schäfer ("Brain," Vol. 10, p. 373) destroyed this convolution on both sides in six monkeys. He states, "Hearing was not only not permanently abolished; it was not perceptibly affected." Luciani ("On the Sensorial Localisations in the Cortex Cerebri, Brain," 1885, p. 154) supports Ferrier, but in doing so includes in his "auditory centre" a large portion of the cortex which Ferrier claims for other functions. Upon which Ferrier indignantly observes, "Luciani appears to contend for a form of localisation which is no localisation at all, as every centre seems to perform to some extent the functions of every other centre." Kirkes in his "Physiology," nine years later, says:—"The auditory area was localised by Ferrier in the superior temporo-sphenoidal convolution. But there is considerable doubt whether this is correct; it is so much more difficult to tell when an animal is deaf than when it is blind." Starling says in his "Physiology":—"The sense of hearing is located by Ferrier in the superior temporo-sphenoidal lobe. Stimulation of this part causes pricking of the ears. Destruction of it produces different effects, according to the experimenter. "With regard to tactile sensibility," says Ferrier, "many uncertainties still exist as to the paths and centres of the various forms of common sensation." The usual endless experiments on all sides, with conflicting statements, theories and controversies have taken place. Kirkes ("Kirkes's Handbook of Physiology," 4th Edition) says, nine years later, "tactile sensibility was localised by Schäfer in the limbic lobe, but there is so much doubt about this that a query is placed after the words 'tactile sense'" in the figure given. Munk and Bastian and Mott and numerous others hold that the area of the brain known as the Rolandic centre, the so-called motor centres of Ferrier, is really the seat of tactile sensibility. But this is stoutly contested. And Professor Schäfer considers that the sensory symptoms obtained by other observers are "due to the general disturbance of the whole brain produced by the severity of the operation" (*Ibid.*). Moreover, he thinks that "the exact localisation of the tactile areas must be left to the future, as in his most recent experiments he has failed to confirm his earlier ones on the gyrus fornicatus." With regard to the senses of smell and taste, Ferrier, from experiments on animals, localised these in the temporal lobes. Schäfer and Sanger-Brown ("Phil. Trans., B 30, 1888), after destroying these lobes, found no impairment or loss of the senses of smell and taste. And Ferrier found, from further experiments, that the losses of these senses which followed upon destruction of those he had regarded as centres, were presently, in part at all events, regained. Kirkes says:—"Similar uncertainty exists as to the situations for taste and smell."

5289. I am afraid this evidence loses its value to us very much by being given in this shape. This is not so much your evidence as a collection which you have made from different authors of what those different authors think?—No, it is a collection of experiments which have been made by authorities upon the brain, and the different conclusions at which different men have arrived.

5290. That is what I endeavoured to say?—Does not that show that vivisection is a useless practice if it results in different effects?

5291. I do not mean to say that this evidence is immaterial, but I say that to read to us what would be an interesting essay upon the subject is not what

witnesses are called for. Witnesses are called to point out, from their own knowledge and experience of a subject, facts and deductions to be made from those facts; and if you would come to your *précis* you say that you are prepared to offer a criticism of certain experiments on animals described in the "Journal of Physiology" for August, 1906. That is a definite matter upon which we can hear your own criticisms, and we can refer to the book and ascertain whether there is anything to be said on the other side about it?—I have taken great pains to prepare these examples which show the conflicting results of physiological experiments. It seems to me that it really is important, because if one set of physiologists experiment on creatures and get certain definite results, and then another set of physiologists experiment and get different results, does not that show that experimental physiology is useless?

5292. I hardly think it shows that, because you might say the same about medical science itself. You might say that different sets of doctors at different times and under different circumstances have formed different opinions as to what were the proper methods of treatment of disease or what the symptoms of disease were, and what the causes of diseases were; but that does not show that medicine is a thing that is futile and useless. You have dealt with the first head of your *précis* I understand?—Yes.

5293. Then now we will pass to the second. You refer in your second head to certain experiments described in the "Journal of Physiology" for August, 1906?—Yes.

5294. Would you refer to those and tell us what your observation upon them is?—I will now, with your permission, consider some of these so-called scientific experiments which are being done to-day in physiological laboratories. Let me take the "Journal of Physiology" for August of last year as a record of recent achievements. And first let me venture to remind you that science is the study of natural law as revealed by its normal and abnormal phenomena. But artificial phenomena, which are manufactured in the laboratory, are not deviations from a natural law. They are, for the most part, purely arbitrary and artificial manifestations which have little or no relation to the normal working of natural laws. For example, to study the growth of a plant in its normal environment and under varying natural conditions, is scientific observation. But to study the plant watered with Epps' cocoa or with Bass' ale, or with guinea-pigs' blood is *not* a scientific experiment, and the deductions made would not be worth the paper on which they were written. Otherwise, our science would be incomplete until we had studied the effects upon all plants, of watering not only by every known fluid under the sun, but also by every possible combination of every known fluid under the sun. I admit that the experiments of modern physiologists *simulate* scientific experiments by the precise and painstaking methods attending them. But we must not allow ourselves to be deceived by these methods. We must judge them by their aim, by the intelligent and productive or by the unintelligent and unproductive idea inspiring them. So judged, I think you will find that the vast majority of them are of the nature of that pseudo-science of the sixpenny magazines, which tells us that, in the course of a lifetime, a man eats as many sheep as would fill a barn of given dimensions, or as many currant buns as, laid edge to edge, would pave Trafalgar Square. I will now take as an example "The Post-mortem Flow of Lymph"—a paper by Mr. F. A. Bainbridge, from the Gordon Laboratory, Guy's Hospital, on page 275 of the "Journal of Physiology," for August, 1906.

5295. Was that an experiment on a living animal?—There was an experiment on a living animal first; they injected something into the blood and then they killed the animal.

5296. The injection was under anæsthesia, and the animal was killed whilst in a state of anæsthesia, was it?—I have not gone into that. I am not dealing with the cruelty question now. I am dealing now with the futility of these experiments. Here are a number of experiments to decide what is the source of a flow of lymph which has been seen to take place from the thoracic duct in animals which have been killed a few minutes after the injection into their veins of a concentrated solution of dextrose. It appears that

Asher believed that this lymph could not be formed by filtration through the capillary walls, but was due to secretion of glands. Mendel and Hooker, who had obtained the same post-mortem flow of lymph after injecting an extract of strawberries, believed, too, that this post-mortem lymph was derived from the tissues. But neither Asher, nor Mendel, nor Hooker, made any observations on the venous pressures. Mr. F. A. Bainbridge, of the Gordon Laboratory, Guy's Hospital, considered, therefore, that Asher's explanation of his results was unproven. Accordingly, he performed a number of experiments to clear up the mystery, the mystery, be it remembered, of a change which took place after death under an altogether abnormal condition—a mystery which could not have the slightest scientific value or practical bearing upon medical treatment. Finally, after a series of elaborate experiments, ingenious, I admit—but so are the operations of the juggler—placing cannulæ in the splenic and external iliac veins, connecting these to manometers filled with sodium sulphate solution coloured with methylene blue, killing some of the animals by injecting chloroform into the jugular vein, others by passing a strong electric current through their hearts, in order to prevent the formation of clots in the heart and interference with the pulmonary circulation, Mr. Bainbridge came to the following conclusions: that "the post-mortem lymph flow after the injection of peptone is due either to filtration through abnormally permeable capillaries, or to increased metabolism in the liver, the former view being the more probable." So that for the life of him, after all his elaborate operations, he is unable to settle this question of the source of the lymph flow after death. It may be due to increased metabolism in the liver. It may be due to mere oozing through the capillaries. All he can say is that previous observers were probably wrong, and that glandular secretion has nothing to do with it. And of what possible interest or practical value would it be if he could tell us? After death the body no longer obeys the laws of life. It becomes subject to those which govern inanimate matter. If tears ooze from the eyes, saliva from the mouth, or lymph from the thoracic duct, it means only that, although life has ceased, a faint flicker of it still lingers in the tissues.

5297. You are now speaking in your capacity as a Licentiate of the Royal College of Physicians from your medical knowledge?—Yes.

5298. And this gentleman, Mr. Bainbridge, I understand, is a gentleman of some skill and reputation as a physiologist. You are now expressing the opinion that he has been following out an experiment which is quite useless?—He got no results.

5299. (Colonel Lockwood.) That is what you contend?—Yes.

5300. (Chairman.) You say that his results were useless?—No, not so much that his results were useless, as that he got no results. He started on the assumption that the other men's results were wrong, and he himself got no results either—no definite results. So that you may go on endlessly and endlessly and endlessly.

5301. You point that out, and you express that view, and there are gentlemen here who will ask you some questions about that, I daresay. Is there anything else you wish to say to make it more clear why, in your opinion, it was a useless experiment or one having no result at all?—Moreover, if he had succeeded in his experiment—had shown conclusively that Asher and Mendel and Cohnheim and Hooker and other observers had been wrong in their conclusions, to what purpose would it have been? Had he absolutely proved by his experiments some hypothesis absolutely different from earlier observers, he would yet not have proved it. For the earlier observers would have already proved the opposite. All he has done is to supply us with further evidence—though this was not needed—that experiments on animals are utterly misleading and unreliable, and, moreover, that physiologists, if they were not well aware of this unreliability, would not, as they do, spend their time in refuting one another's conclusions. May I read another experiment?

5302. Yes, if you please?—This, again, is from the "Journal of Physiology" for August, 1906, at page 282: "The influence of organ extracts of cold-blooded animals on the blood pressure of dogs," by Dr. Orville

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Harry Brown and Don R. Joseph, from the Department of Physiology, St. Louis University. As it had been shown by numerous investigators that when you inject an extract of most of the organs of warm-blooded animals into the veins of other warm-blooded animals, alterations take place in the blood pressure, the idea occurred to Dr. Brown and Don R. Joseph to see what would happen to the blood pressure of such animals when an extract of the organs of cold-blooded animals was injected. Neither claims the credit of the idea. It is stated modestly "the curiosity of one of us was aroused" regarding these effects. As good luck had it, the senior author of the paper chanced to be doing research work at a Marine Biological Laboratory, and so had his materials ready to hand. They removed the organs of various marine creatures as aseptically as was possible. They put them into a clean vessel and minced them finely. I will not weary you by detailing the elaborate processes through which they passed them, but finally they made of them what they term "warm extracts," "boiled extracts," and "sediment extracts," according to the temperature and other conditions under which they were prepared. At last they had them ready, "boiled," "warm," and "sediment" extracts of shark liver, of shark kidney, and other organs, of squeteague liver, of dogfish liver, and of starfish ovary. Then they took living dogs, and making minutely accurate records of respiration, blood pressure, and time of injection, injected these "extracts" into their blood.

5303. (*Colonel Lockwood.*) Under anæsthesia?—I suppose so. I am not dealing with the question of cruelty just now. They made "control" experiments, that is, they at the same time injected into other dogs a saline solution without any extract of fish in it. These saline injections, they tell us, were "without any noteworthy effect."

5304. (*Chairman.*) Does the operator state in this account what the object of these experiments was?—He says that his curiosity was aroused to see what would happen.

5305. I presume that he said something more than that?—I think not.

5306. (*Dr. Gaskell.*) If you read the first page you will see that he says "knowing that organ extracts of warm-blooded animals had such decided effects upon the blood pressure"?—Yes, I said that.

5307. (*Colonel Lockwood.*) Were these experiments done in America?—Yes, in the St. Louis University. They made repeated injections of the shark and squeteague extracts, sometimes of the "boiled," sometimes of the "warm," sometimes of the "sediment" extract, into a number of dogs, always, they state, with the same effects. They illustrate their paper with careful tracings of these effects upon the blood-pressure. In the "conclusions" in which they sum up at the end of their paper let us see what these effects were. Conclusions: "(1) Extracts of the liver, kidney, and sex gland of shark, liver and sex gland of squeteague, liver of dogfish, and ovaries of starfish, contain substances which, when injected into the veins of a dog, cause a lowering of arterial tension. (2) Extracts of the liver and sex gland of shark, and liver of dogfish, and the sperm of starfish contain a variable amount of a substance which raises the arterial pressure of dogs when given intravenously." Here now we get diametrically opposite effects. "(5) The period of action of the depressor substances usually begins during the injection, or immediately afterwards, and endures from twenty seconds to five or six minutes, or even longer. (6) The pressor agent of all the extracts invariably produces an initial and temporary rise of blood pressure, occurring during the period of injection, and in the case of the liver and sex gland of shark and the liver of dogfish a secondary rise is produced, which follows the fall, and which endures usually for a period of four or five minutes. (7) The authors are of opinion that in the case of the extract of the sex gland of shark the active pressor substance, if separated from the depressor bodies, would in a measure simulate the action of adrenalin chlorid. It is indicated that the action of this substance might be more enduring in its action than that of the suprarenal gland. (8) In the cases of the extracts of the liver and sex gland of shark the results of first injections differ from those of subsequent injections in that the secondary pressor effects are usually absent in the first administration, but appear prominently in all the others. (10) In the shark liver extract there is a sub-

stance which depresses respiration. (11) In the shark sex gland there is a substance which stimulates respiration. (12) During the fall of arterial tension the frequency of respiration is usually increased, probably as a result of less blood circulating through the brain. Shall I give all these conclusions? They are all set out in print.

5308. Yes; but they are so conflicting that they prove nothing at all.

5309. (*Chairman.*) I think these gentlemen must have said something more than that they wanted to satisfy their curiosity, because on the first page they say "knowing that organ extracts of warm-blooded animals had such decided effects upon the blood pressure, the curiosity of one of the authors of this paper was aroused regarding the effects of organ extracts of cold-blooded animals upon the blood pressure of warm-blooded animals." I am not a physiologist myself, and I do not know whether this is material, but it may be that physiologists will say that it was material and useful. You say that that is not so—that there is no possible use that that experiment can serve?—I say that the conclusions are so conflicting and so diverse that there is no value in the whole thing. The whole thing is moonshine; there is nothing in it.

5310. You mean because other physiologists have come to different results?—No, I mean because of the different results they came to themselves.

5311. (*Dr. Gaskell.*) Would you tell us where they are conflicting?—For instance, as to the extracts of the liver and kidney, and the sex gland in the first and second conclusions.

5312. Do you notice the word "variable"?—"contain a variable amount of a substance which raises the arterial pressure of dogs when given intravenously." In other words, there are two substances there, a small amount of the pressor substance and a larger amount of the depressor substance?

5313. (*Chairman.*) However, I understand you to say that on the face of it this is a useless and unnecessary experiment?—Yes.

5314. (*Dr. Gaskell.*) I do not see where the "conflicting" is; that is what I want to know.

5315. (*Chairman.*) I daresay some questions will be asked you about it, and there are other witnesses coming probably who will be able to speak to these experiments. You have called our attention to it, and I understand you to say from your physiological knowledge that it is useless?—Will you allow me to go on a little further and finish this about blood pressure?

5316. If you please?—With regard to this same subject of blood pressure, during the recent proceedings of the British Medical Association Dr. Percy Dawson, discussing blood pressure in relation to disease, observed: "There is, perhaps, no one factor which has done more to throw into confusion the literature on the subject now before us than the use of the term blood pressure without specification as to the sort of blood pressure under consideration. The necessity for accuracy of expression can be realised only when it is borne in mind that the systolic may exceed the diastolic pressure by as much as 100 per cent. of the latter, and that these two pressures may vary quite independently of each other." According, therefore, to Dr. Percy Dawson, these experiments of Dr. Brown and Don Joseph, because they do not distinguish between systolic and diastolic blood pressures, are absolutely worthless, even from the laboratory point of view.

5317. It is difficult to know how far you are expressing your own opinions and the opinions expressed in the paper. Is that your own opinion that you are expressing now?—Dr. Percy Dawson, speaking at Toronto in the British Medical Association's proceedings, said this about blood pressures —

5318. I understand that there is great variation between systolic and diastolic blood pressures?—Yes, and these experimenters do not distinguish between systolic and diastolic pressures; so that even if they had got any results, those results, according to Dr. Percy Dawson, would not have been worth considering.

5319. (*Sir John McFadyean.*) Are you sure that the difference between systolic and diastolic blood pressures is not indicated in their diagrams?—I think not. I looked for it. No mention is made at all of it.

5320. (*Chairman.*) You are quite right to express your own opinion that it is useless; but Dr. Percy

Dawson does not say so in so many words. It is your inference from what he says that they are useless.

5321. (*Mr. Ram.*) Is the remark about the uselessness your deduction from the evidence, or is that statement made by Dr. Percy Dawson?—Dr. Percy Dawson was not alluding to these experiments; he was speaking generally of blood pressures. He says when you mention blood pressure and do not distinguish between the systolic and diastolic it simply results in this:—"No fact has done more to throw into confusion the literature on the subject."

5322. (*Chairman.*) You were going to refer to another experiment, I think, under your head 2?—Yes. This is an experiment on the suprarenal gland. In the August number of the "Journal of Physiology," page 332, there is a paper on "The Cortex and Medulla in the Suprarenal Glands," by T. R. Elliott, George Henry Lewes, student, and Ivor Tuckett, from the Physiological Laboratory, Cambridge. A number of experiments were made on guinea-pigs, rabbits, cats, and rats. The aim was "to analyse both in bulk and in cellular detail some of the changes of the suprarenal glands under various conditions," and to determine the relation between the medulla and the cortex. "The evidence obtained," the writers say at the end of their paper, "is ambiguous." Put into plain language the results are so various and so conflicting, not only in different animals, but in animals of the same species, that after reading the paper we feel only that still a little more fog than before surrounds these much investigated glands. The only certain conclusion arrived at, is that when the suprarenal gland of one guinea-pig was transplanted beneath the skin of another guinea-pig, it caused "intense irritation of the surrounding tissues." But the same graft planted beneath the skin of a rabbit, a cat, and a rat had but little effect. It was calmly absorbed. So also, when a rabbit's suprarenal gland was planted in another rabbit, and a rat's in a rat, nothing happened. But other skilled observers had already made this notable discovery. What value under the sun it has, goodness only knows! Then, it was suspected that adrenalin (which is an extractive of the gland) might be the cause of the irritation and edema. Accordingly adrenalin was injected subcutaneously into a guinea-pig, killing it in an hour. Then a fraction of a milligram was found to cause no edema, but the skin over the injection became discoloured and sloughed away. Then, to lessen the rate of absorption, one milligram of adrenalin in salt solution was enclosed in a capsule and placed beneath the skin. This caused no edema or irritation, but slow discoloration and necrosis. Then adrenalin, in the form of solid tabloids of hemisin, was placed under the abdominal skin. The guinea-pig died in three hours—but still with no edema or solution of the tissues, which was what the experimenters were looking for. Then the entire gland of a cat was placed under a guinea-pig skin. No edema or irritation followed. But in a week the skin sloughed away. But a very large gland from "a cat that had recently suckled kittens," they tell us, "killed a small guinea-pig in 36 hours." Yet still there was no edema. Then fragments of a dog's gland, which equalled the mass of a guinea-pig's gland, were tried upon a guinea-pig. Still no edema followed. Nor was edema obtained by the introduction of portions of bullock's and sheep's suprarenals, although a quite small gland from a rabbit produced the much sought for edema, and killed in 20 hours. The gland of a French guinea-pig caused edematous effusion in a British one (apparently the *entente cordiale* is, at all events in guinea-pigs, merely skin deep).

5323. That kind of observation does not help us in our inquiry. I do not mean merely that last observation, but you are treating us to what sounds rather like a partisan description?—But if an experiment will not bear the tests of common sense and humour what is to be said for it?

5324. I am not complaining of the last observation; that was the last straw only. I thought you were treating this throughout in a description which did not profess to be a scientifically accurate description?—Yes, this is distinctly accurate.

5325. We must examine that for ourselves?—Yes, what I want to do is to show the varying results, so that you get no result which can be depended upon. Because you get one result in one creature and another result in another. Then how can we apply these

results to human beings? The glands of one hedge-hog killed a guinea-pig in less than 30 hours. While on the same day the glands of another hedge-hog caused in 30 hours only slight edema. Finally, the writers say of what they themselves describe as "these somewhat tangled restrictions," "The experiments are inadequate to distinguish whether these differences should be ascribed to a specific cytotoxin, or to different conditions of temperature and circulation. A patent fact is that the hæmorrhage and 'exhaustion' of the suprarenal glands are not due simply to the introduction of another suprarenal into the animal's body; for were such the case, it would be more severe with the rapid absorption from the peritoneal cavity. It must be a secondary result, determined by the necrosis of the superficial tissues and by the resultant disturbances, whether mechanical or chemical, in the animal's economy." In other words, all these symptoms they had been so carefully observing and describing, were due merely to blood-poisoning, arising from the decomposing gland which had been placed beneath the skin. Of the relationship of medulla to cortex, which was the aim of the experiments, the writers are able to tell us nothing. Of the great development of the cortex which is seen in the mammal the writers say:—"To speculate on its meaning would at present be idle." The writers point out that its function is said by many scientists to be the neutralising of various toxins. Myers published in the "British Medical Journal" a definite experiment "to show that suprarenal extract can neutralise cobra poison." On the other hand, however, although the suprarenal glands are injured in diphtheria poisoning, Mr. Noon, of the Lister Institute, made some experiments on this point for the writers of the paper, but "he could find no proof of the neutralisation of diphtheria toxin by suprarenal extract." In point of fact, the only result of the experiments, and of the time and energy, and the suffering and the sacrifice of life, was to show, as the authors state it, "that the subcutaneous tissues of the guinea-pig are peculiarly sensitive to suprarenal grafts." But who in the world cares whether they are, or are not? The showing has neither a scientific interest, a bearing on medical treatment, nor a vestige of practical value. It is no contribution even to our knowledge of the temperament and nature of the guinea-pig. It is a mere superfluous observation of a personal antipathy on the part of a guinea-pig's subcutaneous tissues to have nasty little bits of suprarenal gland grafted into them. May I read something about the thyroid gland?

5326. Could you not tell us shortly as regards the thyroid gland; what the object of the experiment was, and then tell us without unnecessary comment that it failed entirely in its object?—There have been so many experiments on the thyroid and parathyroid glands, and they all conflict. One observer comes to one conclusion, and another observer comes to an absolutely different conclusion.

5327. I find it quite impossible in following you to understand how far what you are stating is something taken from a book or is part of your own criticism upon it. Could you not tell us what the object of the experiment was and what the result of it was, and point out that it was in your opinion useless without reading from a paper in which you repeat over and over again on each experiment the arguments about the uselessness generally of physiological experiments?—Well, these thyroid glands are very important. For years they have been experimenting on them.

5328. Yes, we have heard a great deal of them already?—And if one experimenter gets absolutely different results from another, surely that shows the valuelessness of vivisection—that you cannot depend on its results. If you get varying results in the same species of animal, how much more likely are you to get varying results when you try to apply them to human beings?

5329. (*Colonel Lockwood.*) We may take it for granted from the witness, may we not, that she thinks that these experiments have led to no definite results; we may take that as her opinion?

(*Witness.*) No, not as my opinion. I take their own results at one time and pit them against their own results at another.

5330. (*Chairman.*) I certainly thought we were taking your opinion as a physiologist upon it?—No.

5331. As a person skilled in medicine?—No, I think

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not. I have brought the evidence of the men themselves to show it.

5332. (*Colonel Lockwood.*) It is your literary deduction we will call it, from their own writings?—It is their own deductions.

5333. We will take it from you that you think that is the case.

(*Sir John McPadycan.*) Might we not go further than that and declare ourselves agreed that different experimenters have on many occasions in pursuing the same problem arrived at different conclusions; and also might we not express ourselves as perfectly satisfied that in spite of experimentation, there are many problems connected with pathology and physiology that are absolutely obscure? I need no evidence to convince me of that.

(*Witness.*) Then how are we to depend upon those experiments?

5334. If you will allow me to answer that by another question, will you tell us what method of investigation cannot be discredited in the same way? If we are ignorant about anything and all other means of investigation that you can tell us about have been available, presumably they have been employed, and have not discovered the truth, and therefore they are discredited just like vivisection according to your logic?—But I think if you examine the physiological books you will find that all our results, or the most important of our results, have been deduced from pathological observation and from subjective and clinical observation.

5335. But according to your own account the whole of them have been futile?—No. I am only trying to show the futility of the experiments on animals.

5336. But it is admitted I think by all of us that we are very ignorant about the functions for instance of these ductless glands; but will you tell us why it is that the same train of reasoning does not discredit clinical observation and post-mortem observation?—I think we are neglecting post-mortem observation and clinical observation.

5337. Can you give us evidence to show that it is being neglected?—Yes, I can give you evidence. May I not read my statement on the thyroid gland?

5338. (*Chairman.*) I think it is unnecessary if you are endeavouring to show that many experiments have been unsuccessful, and that many experimenters have arrived at different results from others at different times. I suppose that is quite possible whether you are dealing with clinical observation or pathological or physiological or whatever science it may be?—I think the different results and the conflicting results arise, because it is impossible to deduce any true conclusions from the conditions of an animal in a state, in the first place of anæsthesia, and in the second place of mutilation—that these entirely stultify any results that we may get, because they place the animal under entirely abnormal conditions.

5339. If you say that that is your opinion—and it may be quite correct—it is admissible as your opinion upon that point, but is it your opinion as a person of some medical knowledge?—Yes, as well as it is my opinion gathered from reading the experiments of these men who get entirely conflicting results. I think very likely—this is only my personal opinion—but, if one knows anything of the phenomena of hypotism, one cannot help thinking that very often these animals must be entirely under the hypnotic influence of the man who operates. Consequently he gets what he expects to get; and another man seeking for something entirely different in the same way gets entirely different results.

5340. It may be that you are perfectly right in your deductions. I am not saying that you are not; but to say that that proves that no experiment can be of any value, I think we should all consider is saying too much. Your general view of the matter I think we quite understand, and I do not think anybody is prepared to dispute that you will be able to find different experimenters who have drawn different deductions at different times from the same facts?—You see I have taken all my experiments from one magazine, and that is a very fair example of all the other magazines. And if all their results are so conflicting why do we allow this constant torture to go on with such miserable results?

5341. (*Chairman.*) Perhaps you will come now to the third head of your *précis*. I understand from your *précis* that you object to the principle of obtaining immunity from microbic infection by inoculation?—Yes.

5342. And that you consider that it is unscientific as well as futile?—Yes.

5343. The futility of experiments I think you have dealt with generally, but why do you say that it is unscientific?—Because I think the plan of nature beneficent. Nature evolved us from the lower animal into man; therefore the plan of nature must be beneficent. And we are attempting to divert and thwart the operations of nature without understanding them—we admit we do not understand them. Disease is a natural operation of nature, and when we attempt to thwart the operations of nature I think we are putting back evolution.

5344. Then you mean that you think that attempts to inoculate to prevent disease are wrong?—They are wrong.

5345. Because it is against nature?—Not only because it is against nature.

5346. I thought you said we were fighting against nature?—Not only because it is against nature, but also because we are putting into human blood the blood of one of the lower creatures. And there is evidence to show that when we put the blood of one of the lower creatures into the human being we confer not only the immunities of that lower creature, but the susceptibilities of that lower creature.

5347. We will separate those two, because you said, and you say still, that it is against nature, but also you say it is acting as a poison on the person?—Yes.

5348. As regards its being against nature, you mean when you say that, I presume, that nature intended children to have the various diseases, for example, to which you refer in your *précis*?—Yes, I think there is no other way to get out of it. I do not see how else the unvarying occurrence of these diseases can be explained.

5349. That is your opinion?—Yes.

5350. That no preventive means ought to be used to prevent their having these illnesses?—No other preventive means except isolation; no preventive means such as vaccination or injecting serums, for example.

5351. But you would not object to the prevention of infection in the ordinary way?—Certainly not, by isolation.

5352. Or by cure?—Certainly not. Cure is simply rousing the natural resisting power which nature gives to the patient.

5353. Then cure with the assistance of nature, you admit?—Certainly.

5354. Then is it a doctor's duty to save life if he can?—Certainly.

5355. Supposing he has a serum which will save life; is it his duty to use it?—I do not believe in a serum that will save life.

5356. I was asking you to suppose it. I should not ask you that unless there were a great many people of great medical skill who believe there is such a thing. I think the great majority of medical authorities believe that there are curative sera. Supposing that a doctor believed, in concert with the great majority of his profession, that he was possessed of a serum which would cure a disease, would you say that it was not his duty to use it?—No. What I say is that for that which may be a slight disease or slight illness we may entail upon the patient serious and lasting results. For example, Dr. Paton has shown in his "New Serum Therapy" that when he was giving by the mouth, to human patients, the plasmata of various animals and antidiaphtheric serum, he conferred upon the patient the susceptibilities of the animal—the susceptibility, for example, of horses and sheep to influenza and catarrh.

5357. I will come to that. You are now saying a little different from what you said in your *précis*. You say there: "The microbe, making as it does for the survival of the fittest, is a factor as indispensable to the health and evolution of the race as are air and light. For example, the child having been already tested by the severe ordeal of dentition, is further tested for his fitness for survival by the zymotic diseases; the nervous system and respiratory organs by

whooping cough; the skin, throat (pharyngeal), respiratory organs and mucous membranes by measles; the skin, throat (laryngeal), serous membranes, and kidneys by scarlet fever, and so forth. By these means the efficiency (or otherwise) of the constitution is early tested, and the candidate for life turned back, if inefficient, at the outset." That was the ground in your *précis*, which, I presume, is a considered statement. Am I to understand that that is correct?—Yes, I believe it to be so.

5358. Therefore, you do say there that quite apart from whether it is by serum or anything else, the preventive methods are in themselves unscientific and bad?—No, I think only the abnormal preventive methods, such as by injection of serum.

5359. That is what you mean, is it?—Yes.

5360. It is not what you said. You do not use the word abnormal or anything of the kind; you say that these diseases were sent by Providence or by nature for the purpose of testing children; and that it is unscientific to interfere with their operations?—No, but that it is unscientific to inject children with serum with that object, I think distinctly so, because if you make them immune to the microbe you make them insensitive to the deleterious conditions which breed the microbe.

5361. Would you apply the same doctrine to curing a person by serum if it was a grown up person—an adult?—Certainly.

5362. I do not quite see why you put forward the case of children and their diseases being sent by Providence as things that they ought to be tested with?—Not by Providence, by nature. It is part of the plan of a great developmental scheme which we do not understand.

5363. I did not mean to offend any religious sentiment by using the word Providence. I will use your own word nature. That is what your view is?—Yes.

5364. You say that these diseases are sent to test children and therefore it is unnatural to interfere with them?—It is unnatural to try to prevent them by the inoculation of a serum.

5365. Why is it more unnatural to prevent them by the inoculation of a serum than by any other remedy which will cure them or prevent the disease?—Because by the inoculation of a serum you merely confer a tolerance to the circulation in their blood of disease germs which the healthy body should rebel against.

5366. Then that is what your objection comes to—I think I was right in saying that it is hardly what you have expressed here in your *précis*. But your objection is not that it is unnatural or wrong to cure disease or prevent disease?—Certainly not.

5367. Provided, of course, that the method does not involve the use of serum taken from another animal?—Yes.

5368. Or another man, would you say, or human being?—I think the admixture of bloods is a loathsome and dangerous and abnormal practice, and one of which we have no conception of what the results may be.

5369. Your objection would be to vaccination?—Yes. For instance, I say this. As I was saying about Dr. Paton, if he has shown that in conferring the immunity of a creature we also confer the susceptibility of the creature, how can we say that vaccination has not given to children the great susceptibility of calves to tuberculosis?

5370. I understand quite, and I think we all understand quite, what your point is—that you say the same argument applies which you apply against vaccination?—Yes.

5371. And you say it is practically the same question?—Yes.

5372. I certainly did not understand that from your *précis*, but I think I now quite understand what you say. Then your next point is that you wish to plead that to inflict pain on helpless friendly creatures for mere experimental purposes is subversive of the sense of right, of honour, and of humanity; that it is demoralising to those who practise it, to those who witness or read about it, and to those of the community who seek by the torture of innocent creatures vicariously to escape the consequences of their own trespasses against the laws of health. I observe that what you are pleading against there is the infliction of pain and torture?—Yes, and the use of creatures in such a way

as to treat sentient living creatures as mere material for experimental purposes. I think that is a demoralising attitude of mind.

5373. But I suppose if those experiments were things which were agreeable to those creatures you would not object to it. It is, I understand, because they inflict pain and torture?—That is the main objection, of course.

5374. That is not quite what you state in your *précis*—you are not pinned to it, of course, at all; but I thought it was what you meant. You say you wish to plead that it is wrong to inflict pain upon helpless friendly creatures?—May I read a short statement that I have, "A plea for the Preservation of the Humane Sense," as you have come to that?

5375. I think you can answer these questions quite easily, can you not? This is not a question of scientific knowledge; it is a question of opinion?—I think it is a question of sentiment a little, and sentiment is as integral and essential a part of human consciousness as is reason.

5376. We are all agreed that sentiment and feeling have a great deal to do with us in this world, and ought to be considered as much as a great many other things. But I am only pointing out that your objection, as I understand it, to these operations is that they are painful to the animal. If they gave pleasure to the animal you would not see any objection to them, I suppose?—That is a difficult question.

5377. I should have thought it was not a difficult question. I have always understood the objection to such experiments to be because of the pain that they inflict?—I think the objection is to the mental attitude of exploiting these poor creatures which are dependent upon us.

5378. Then you would go further perhaps and say that even if they were painlessly killed you would not object to it?—No, I would not say that.

5379. You would not object to their being painlessly killed?—They are painlessly killed as food for human beings.

5380. That is what I was going to point out to you?—Yes, but the question of torture, the question of exploiting them as it were, for our purposes is contrary to the sense of right, and I am quite sure it can only demoralise those who do it, and those who sanction it.

5381. I assure you I am only trying to get at what you do mean. I thought I had expressed it, and you now tell me that I had, because you tell me that what you object to is the idea of inflicting pain and torture upon animals for our own benefit?—Yes.

5382. That I understand. That does not touch the case of the present law, so far as that law is really effectively carried out by anaesthetics, and without any certificate exposing the animal to torture after the operation is over?—Yes, it does, because as I said before, and as I have tried to maintain, it is not only a question of cruelty, it is a question of the attitude of mind which looks at these poor creatures as subjects for experiment—apart entirely from the question of cruelty.

5383. Subjects for painless experiment?—Subjects for experiment or painless experiment.

5383a. (Colonel Lockwood.) What you say is that we have no moral right to use an animal in that way?—Yes, that is what I mean—that we have no moral right.

5384. (Chairman.) But we have a moral right to kill them for food?—Yes.

5385. Although people can live on vegetables?—Yes, I think when the moral sense is well developed, as it is evolving now, we shall not kill them for food. But that is a question of time.

5386. That would be the consequence of carrying out your views?—Yes, it is a question of time and evolution. These things have to be so gradual.

5387. Then you say that this infliction of pain upon living creatures on the part of men is done vicariously to escape the consequences of their own trespass against the laws of health. I do not quite follow that. If a child has diphtheria, assuming (if you will assume it for a moment, you would not, I daresay, agree to it) that those physiologists and doctors are right who say that the antitoxin used for diphtheria has been, not always, but to a considerable extent, successful in curing it, why should not the child have the benefit of

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that?—I am prepared to show from the report of the Lister Institute that the injection of the diphtheric antitoxin is followed by most dangerous complications. For example, it has increased the number of cases of paralysis, which is a far more serious symptom than the throat symptoms. It causes convulsions, it causes sudden death, it causes rashes, it causes swellings of joints. These facts are taken from the report of the Lister Institute itself, which prepares this antitoxin, and from other authorities.

5388. That is the argument you were last addressing to us. You say that is the reason why you would not have a child cured in that way?—Yes.

5389. But supposing it was not a question of a serum, and supposing experiments on animals had conveyed additional knowledge to a doctor or surgeon which enabled him without administering any serum successfully to treat a disease in a way in which formerly he would not have treated it, why should not a child (I say a child, but there is no question about a child vicariously escaping the consequences of its own trespasses) have the benefit of that treatment?—But, I think, if you examine these things you will find that there is no scientific progress which is not hand in hand with moral progress. Consequently any trespass against morality will never result in a scientific advance.

5390. That is your answer to that. You think that the experiments, though painless, are immoral?—I do.

5391. And consequently that no result from them can be of benefit?—Yes, I say that consequently they have not led to any beneficial result. Because from their very source, their very nature from the beginning is wrong and is immoral.

5392. Then your last argument is a plea that having regard to the subtle nature of the complex processes called "life," it being impossible to dissociate psychical and physiological factors, the crude and unscientific experimental or objective method of research should be abandoned in favour of the more intelligent and scientific subjective and pathological methods. I do not quite understand what the contrast is between the pathological methods and the crude unscientific experimental or objective methods?—In the pathological and the clinical methods we observe patients in hospital, and observe patients on the post-mortem table, so that we compare the diseases of the organs after death with the physical or physiological phenomena which appeared during life. I have not put it very clearly.

5393. Is not the gist of it that you contrast the unscientific experimental method with the scientific pathological method?—Yes.

5394. So that it really comes back to that. If you are right in saying that the pathological is the only scientific method, then the experimental is necessarily an unscientific method?—For example, in disease of the pancreas let us say, if we note a patient during life and find the symptoms which occur from disease of the pancreas, and note the extent of the disease when the patient is on the post-mortem table, then we get a rational idea of the value of the function of the pancreas. But if we produce artificially a disease of the pancreas in a rabbit we lose the psychological symptoms—we lack really the most valuable observation of the absolute function of the pancreas.

5395. Why do you contrast the two? Do you assume that if experiments show certain results a doctor immediately in treating his patient throws away all the results of pathological and clinical research?—I think you will find that that is so; that nowadays we have come to depend so much upon experiments on animals that all the authorities will tell you that clinical and pathological research are being neglected. And those, I think, are the only really valuable means to advance in medicine—and in physiology also.

5396. I must leave some of these medical gentlemen who are here to deal with that. I am no match for you on these medical questions?—Will you let me read my plea for the preservation of the humane sense?

5397. We are all in favour of humanity I hope, but the difficulty is in knowing precisely what are the limits?—Do you remember Cicero's fine words: "Whatsoever that be which thinks, which understands, which acts, it is something celestial and divine, and on that account must necessarily be eternal"?

5398. (Colonel Lockwood.) With all respect to Cicero.

it is evidence we are asking for, not what the gentleman said originally or now. We take it for granted that you plead for humanity?—Do you?

5399. (Chairman.) I think we cannot have an essay on humanity as being within the scope of our inquiry. We ought to be humane, if we are not?—But do you think that the modern medical scientist does consider humanity sufficiently?

5400. I am not a witness, but I dare say we none of us consider humanity sufficiently on a vast number of occasions; it may be that a surgeon does not occasionally. But I do not think that is what we are sitting here to inquire into?—If you read through the proceedings of the British Medical Association at Toronto you will find a very large proportion of the papers read there, were as to the production of disease in animals. Not as to the healing of disease in human beings, but as to the artificial production of disease in animals. And that, I think, is a dangerous tendency.

5401. Those were experiments, I think, conducted in a country where the same law does not prevail?—But these were Englishmen; these were people from every nation who were speaking at Toronto.

5402. That would only go to show that all nations contain inhuman people, in your view. I do not think that is what we have to inquire into?—But I think it is a dangerous tendency—this delight in the production of disease in an animal. If you find the word "success" in a paper before one of these medical associations, you will find that more often that success is a question of success in inducing disease in an animal, than success in healing a human being. I think you will find that is absolutely so if you look into these cases. I have, in fact, among my papers a quotation from the "Hospital" showing by the papers read before an Association—I forget the percentage; it is in my papers—that the subject of treatment is passing into disuse. Again, if you read the "British Medical Journal" on the subject of this epidemic cerebro-spinal meningitis (of which we have recently heard a good deal), you may look carefully to find something about treatment; you will find little or nothing about treatment. It describes the bacterium; how it can be cultivated, whether it grows on certain broths or gelatine—

5403. That is not a thing that can be dealt with by an amendment of the law relating to experiments on animals. It is getting a very long way from that. It is discussing generally the failure of the human race in humanity and other things?—I think we are neglecting treatment because we are expecting to find some serum that will cure everything.

5404. (Colonel Lockwood.) Do you know what the terms of reference to this Royal Commission are? Have you ever looked to see what we are sitting here to inquire into?—Yes. I am not quite sure what they are.

5405. I will just read it to you:—"To inquire into and report upon the practice of subjecting live animals to experiments, whether by vivisection or otherwise; and also to inquire into the law relating to that practice and its administration; and to report whether any, and, if so, what changes are desirable." That is the limit of our inquiry?—Yes.

5406. I understand your objection. The main doctrine that you have been putting before us this afternoon is this, that you object first of all to all experiments on animals?—Yes, I do.

5407. Whether they are anaesthetised or not, you still object to it, do you not?—I think it has been proved to be quite impossible to thoroughly anaesthetise them.

5408. Then I will take it that you object to all experiments on animals whether they are supposed to be anaesthetised or not, because you believe it to be an immoral proceeding. Is not that correct?—Yes.

5409. And that the surgeons are on the wrong lines when they practise these experiments because they ought to seek their ends by other means. Is not that right?—Yes.

5410. Do you know the law as relating to licences issued to men who practise experiments?—Perhaps I do not know it very exactly. I have not taken up the administration of the law.

5411. You do not know anything about the various safeguards that hedge round the operations?—I do not regard any of them as sufficient safeguards.

5412. Therefore I presume you decline to give us any

opinion as to how the law ought to be administered?—I think the only way to alter the law is absolutely to abolish experiments on animals.

5413. You are not in favour of any alteration of the law; you are in favour of the total abolition?—I am in favour of total abolition.

5414. Of any experiments on animals of any sort or kind?—Any experiments, but not observations of animals.

5415. Not clinical observations?—No.

5416. But of any experiments on animals at all?—Yes, of exploiting them for our purposes.

5417. Would you explain to us where you draw the line between observation and experiment?—We observe a patient in hospital, but we are supposed not to experiment on that patient. By observation I mean every day observation of animals in sickness or in health.

5418. I understand from your answers to the chairman that you do not think it is allowable to treat a child suffering, say, from diphtheria or anything else, with any inoculation or serum because it has been obtained, you think, from a lower class of animal. Is not that it?—Yes, because I think we do not understand the dangers there may be, and dangers there certainly are, in this admixture of bloods, of the human blood with the blood of lower creatures. I think it is a danger that has never been fully investigated. Everywhere they are, more or less at random, injecting sera into human patients without following up the effects these may develop in two or three years. For example, if a man is bitten by a rabid dog the wound may heal, and he may be apparently in health for some years. Some of the authorities give the case of a man who was bitten by a dog and was apparently in good health for ten years, when suddenly hydrophobia broke out. How can we say when we inject the serum of one of the lower creatures into a human being what inherency or what disease it may not subsequently develop into? We do not know these things.

5419. You do not extend your remarks to the fact that a person who contracts a disease by his own fault has no right to be cured of that disease?—No, certainly not.

5420. I have heard that theory advanced, that if a person contracts a disease by his own fault you have no right to try and cure him?—That is what we medical people exist for.

5421. You have stated, and proved by your various statements, that there is a great difference of opinion between surgeons and physicians as to the results of various experiments; you have shown us how very doubtful are the researches, in your opinion, up to the present moment?—Yes.

5422. Have you ever known any scientific question as to which great men have not differed until a law was discovered?—But these are not questions of opinion, they are questions of fact. If you remove the thyroid or parathyroid glands from an animal, and you observe certain results—myxedema or tetanus—that is a question of fact. And if another person removes the thyroid or parathyroid glands, and finds that no symptoms follow, there again is a question of fact; and these two questions of fact put one against another neutralise one another.

5423. I take it for granted that you are right, and will acknowledge for your purposes that men do differ?—Not as a question of opinion, but as a question of fact.

5424. As a question of fact that they do differ?—Yes.

5425. Have you ever known any great science where the men who followed that science have not differed upon points of fact?—For example, when Newton, sitting in an orchard, saw an apple fall to the ground, he deduced from that the law of gravity. Nobody has I think refuted that.

5426. (*Sir William Church.*) In the evidence which Mrs. Cook gave before us she speaks of you, in answer to a question from me, as being “an expert who will follow me on that subject.” I was asking her some questions which were founded on what is called, I think, your Open Letter?—Yes.

5427. I did not quite understand at the time, neither do I now, what the meaning of your being an expert is?—I think it was only a compliment on her part—

simply that I was a medical person; that was all she meant. I disclaim absolutely the title of an expert.

5428. Therefore there are only a few questions that I should like to ask you. First of all, I think you wish your Open Letter to be taken as part of your *précis* of evidence?—Yes, if you please.

5429. Then I should first like to ask you the grounds for your statement that “the present great object of medical science is to discover in the blood of one or another of the lower animals ‘cures’ for all ills, or semi-miraculous substances which, injected into the blood, shall render us ‘immune’ to diseases we have incurred by neglecting natural laws.” What did you mean by that? Is that really the great object of medical science?—I think that medical science is devoting itself to-day to seeking sera to cure all forms of disease—that is, “semi-miraculous substances.”

5430. Is it not really the case that the great object of medical science is to endeavour to acquaint ourselves with the natural history of diseases, and to see how they get well naturally, and to try and make use of what information we can acquire of the way in which diseases naturally get well, and see whether we cannot use those means to assist and quicken the efforts of Nature in getting well?—I scarcely think so. I think that nowadays treatment and healing by drugs and such natural methods are being neglected—and the study of them.

5431. But you disclaim any knowledge as an expert. You have not worked in a physiological or pathological laboratory to any great extent?—No, I have not.

5432. Then perhaps it is a waste of time to ask you how we think now that some of what you have called in your paper the zymotic diseases come to an end in our bodies. You are not prepared to answer that?—Yes, I am prepared. I have studied the subject, and I know a good deal about it, I believe. I think, with all due submission, that none of us can boast of knowing very much about these subjects.

5433. And you think that we ought not to try to find out?—No; I do not think that, certainly. I think we ought. But we do not, for instance, sufficiently study types of people. We might gain a great deal of knowledge by studying the types of persons who get certain diseases.

5434. I want to know whether you think that really that is the principal thing that medicine is now concerned in—to try to find out semi-miraculous substances to cure disease?—Distinctly, I think that is the great aim of modern medicine.

5435. But what is your ground for saying that, further than your own imagination?—I study the medical journals. I study the proceedings of the British Medical Association, and I study books on the subject.

5436. You mean by that that this particular portion of medicine is at the present time occupying a great deal of attention?—Yes, to the neglect of more valuable methods.

5437. But it is a very strong statement to make, that the present great object of medical science is to establish semi-miraculous substances to cure disease?—I do not think it is a bit too strong. I think the ordinary medical scientist nowadays has no faith in anything but sera.

5438. Do you mean to say that nothing has been done of late years in any other direction?—I think very little has been done, comparatively. I think that modern medicine is more or less a study of the microbe and the habits of the microbe.

5439. I do not quite know what you mean by the study of the microbe?—Bacteria—micro-organisms.

5440. Have we not absolute knowledge that micro-organisms of some sort or another are, at all events, associated with, if they are not the cause of, a great many specific diseases?—I think we have not proved that any micro-organism is the cause. We know that it is associated with it, but that may be merely incidental. The proof is not sufficient that it is the cause. I think that is generally admitted.

5441. You surely will admit—I will put it in that way—that the micro-organisms are associated with certain specific diseases?—Associated, I admit.

5442. You do not admit that they are the cause?—No. Dr. Granville Bantock will come before you and

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will give evidence before the Commission, and I think he will show you that there is very strong evidence that the microbe is absolutely not the cause of the disease.

5443. Then you are not satisfied that the tubercle bacillus is the cause of tubercle?—I am not satisfied. It is an incident in the disease, but I do not think we know what part it plays.

5444. I take that as your answer. You do not think, for instance, that the bacillus of typhoid fever is the cause of typhoid fever?—It is present—not in all cases of typhoid fever—so that we cannot take it to be the cause.

5445. You do not consider that it is the actual true cause?—I do not think that we know enough to say that it is.

5446. I am quite content with that answer. Do you know anything about the organism found in glanders. say; do you not agree that that is the cause of glanders?—I think that what is true of one organism must be true of all; that our knowledge at present is only sufficient to show that they are incidents in the disease, but that we have not knowledge sufficient to show that they are the cause in any disease.

5447. I accept that, of course, at once as your opinion. Have you any doubt that in these diseases the way in which natural cure takes place is by the gradual presence in the blood of something which acts in a contrary way to the toxin produced by the organism?—I think this has not been proved. I know a number of authorities who do not believe in the antidotal action of the antitoxins at all; they hold that disease is cured by oxidation and by the action of the living cell.

5448. I do not care for the opinion of other people; I wish to get your opinion?—I dare not advance my opinion before this Commission.

5449. You do not pose as being an expert?—I do not claim to be an expert, but I give the opinions of experts who support my view.

5450. Then if you are not an expert ought you not to be rather careful in the statements that you make? I should like to ask you a question on this paragraph in your Open Letter: "Men used to working in sewers become 'immune' to noxious gases and microbes." Is that the case?—I think that is well proved. I think it is an established fact that at first persons who are exposed to sewer gas suffer from sickness and diarrhoea.

5451. Where do you get that information from?—Well, I cannot give you the authority, but I believe it is an accredited fact.

5452. Men get suffocated sometimes in sewers from noxious gases?—I do not mean that they will stand absolutely poisonous gases, but the admixture of noxious gas with the air which is general in sewers.

5453. What did you mean by their becoming immune to noxious gases? You make that definite statement?—I do.

5454. I do not wish you to think that I am in any way hostilely cross-examining you. I merely want to know for my own information what you meant by that?—It is this: that when a man who has been used to fresh air with plenty of oxygen goes into bad air he gets symptoms of a certain description; but if he remains for a long while in that bad air he becomes to a certain extent immune to it.

5455. He gets accustomed to it you mean?—He gets accustomed to it. Exactly. It is some sort of immunity. I mean that his system does not resist it; his system does not rebel against it.

5456. Then you go on to say: "Can we suppose that those noxious gases and microbes have ceased to gravely and steadily deteriorate their tissues"?—I think that is rational.

5457. On what grounds do you think that?—Can we believe that it is as healthy to breathe bad air as it is to breathe good air? If it is as healthy to breathe bad air as it is to breathe good air, why do we take all the trouble that we do about ventilation? It is simply that the men become accustomed to it just as men may become accustomed to bad moral principles.

5458. You know that there have been investigations at different times into the health of sewer men?—Yes.

5459. Had you looked up those authorities before you wrote this paragraph?—No, because I do not know anybody who is a sufficient authority to be able to say what is health and what is not health. The majority

of persons regard muscular fitness as health, but health is the balance of all the faculties, and I do not think there are many who are able to judge what is health and what is not health.

5460. Therefore this statement is made just as a sort of general idea, not from any special knowledge that you have?—I think that is special knowledge. I think it is a well-known fact that all men when they first begin to work in sewers get ill.

5461. Do you know that in the latest work with which I am acquainted in which that is mentioned the matter is summed up thus: "The evidence is, on the whole, opposed to the view that sewer men suffer in health in consequence of their occupation." That is in the last edition of Parkes' "Hygiene," the latest book on the subject. Perhaps you did not know also that there was an investigation into the health of sewer men at the instigation of the Metropolitan Board of Works some years ago; there has been no such investigation held since; but in that case it quite supports your view that they are immune, because, curiously enough, among those who worked inside the sewers there were no cases of typhoid fever, and among those who worked outside the sewers at the outfall works there were a few cases?—If you carry that to its logical conclusion you must admit that we are just as healthy living in bad air as in good air, which, I think, as Euclid says, is absurd.

5462. But when you say that they are immune, I want to know to what they are immune. It appears that you meant by that that they are accustomed to it?—That is what I call immunity. I think that all the immunity we can confer is by accustoming people to noxious elements.

5463. Then I think in a letter which you addressed to Lord Selby you were good enough to say what you meant by your statement about one observer having vivisected 2,000 creatures to substantiate one theory, and another having vivisected an equal number to prove the opposite?—I answered that.

5464. I think in your letter to Lord Selby you said that you were referring to Magendie and Flourens?—But I answered that.

5465. Do you mean to say that Magendie and Flourens only experimented on living animals for one purpose?—No, but that is given as the number they experimented on for one purpose. The story is told—it is a classical story—on the evidence of Blatin.

5466. You say "to substantiate a theory," but you did not mention what the theory was for which these 4,000 animals were sacrificed?—The theory was the function of the posterior spinal nerve roots.

5467. You do not mean to tell the Commission that Magendie discovered that, or that he had only experimented on living animals for the purpose of finding out whether it was so?—No, he did not discover it, Sir Charles Bell discovered it, and Magendie performed his first 2,000 experiments to show that it was true, and his second 2,000 experiments to show that it was false, and Flourens some further thousands of experiments to show again that it was true.

5468. You said that it was a classical instance. I confess I never heard it myself as the work of either Magendie or Flourens. I know that Magendie at first doubted the truth of Sir Charles Bell's discovery, and that he did submit a large number of animals to experiment. I doubt whether he ever submitted 2,000 to experiment for that particular purpose. What is your ground for saying that he did?—It is a well-known story, and it has never been contested.

5469. In the same way as the immunity of sewer men was a well-known story. You have no authority for it?—I have no authority except the authority of Blatin.

5470. Do you really think it is right on a serious matter like this to put forward all these general statements for which you have no authority at all?—It is given on Monsieur Blatin's authority. He told that story. It is not on my authority; it has been stated over and over again, and it has never been contested.

5471. (Chairman.) He told it in print, do you mean?—Yes.

5472. (Dr. Gaskell.) Can you give us the reference?—The statement occurs in "Ayez Pitié: Quelques mots sur l'Urgence d'abolir totalement la Vivisection."

tion," by Jules Charles Scholl. 1 Vol. Lausanne: Imer et Payot, 1881.

5473. (*Sir William Church.*) Do you think that in a serious communication to us as Commissioners here, you are quite justified in making these statements which are founded apparently upon general report?—That is not general report; it is given on Blatin's authority.

5474. Then you told us in your letter to Lord Selby that you halved the number. Does the passage in the authority to which you refer say that these experimenters used 4,000 animals?—No, but as I was not mentioning names, I halved the number in order to be within the truth.

5475. It is of no importance, because it is not to the point, as those experiments were done 60 and 70 years ago?—Yes, but if experiments done 60 and 70 years ago could prove a point by vivisection and then disprove it by vivisection, surely that is a very strong argument against vivisection?

5476. Then you made a singular remark, I think, in your letter to Lord Selby which I do not quite understand: "I was compelled to cite experiments of the last generation as modern experimenters are reticent upon this particular." What did you mean by that?—I mean that if you take any of these magazines, the "Journal of Physiology," for example, they do not start by saying how many animals were sacrificed to certain experiments.

5477. But the numbers are published every year by the Home Office?—But one does not know how many were sacrificed to one particular experiment, therefore I say that they are reticent.

5478. They may not state the number in their paper, but you can know how many animals they have used in the year by the returns to the Home Office?—I think it would be more candid and straightforward if every experimenter in describing his experiments were to say, "We used for this experiment so many cats and so many dogs" so that we could to a certain extent gauge the basis of their experiments.

5479. They generally do tell you how many experiments they make use of?—But not how many creatures; that has been admitted before this Commission, I think.

5480. Are you not aware that you have to get a fresh animal for each experiment if you have a licence?—I think it has been admitted before this Commission that the number of experiments does not necessarily set forth the number of animals experimented upon.

5481. In the case of a dog, except when you are experimenting under certificate B, the animal has to be killed at the end of the experiment?—I am quite sure you will find in the report of these proceedings that the number of experiments does not necessarily tell you the number of animals used for each experiment.

5482. Do you know the form in which a person has to apply for a licence?—I will not speak of the licence, I will simply speak of the journals in which these experiments are recorded.

5483. Do you think that when a person communicates his paper to a scientific journal he should be obliged to give information to you? He is writing for the benefit of other scientific men?—Would it not be to the benefit of those scientific men and to the benefit of science in general if he were to mention the number of animals used?

5484. (*Dr. Gaskell.*) If there is any importance in that statement, they always do?—They give the number of experiments, but they do not say the number of animals to each experiment.

5485. In the case you have mentioned of thyroid and parathyroid glands, on casually opening it I see it says two badgers were used, for instance?—If you go all the way through I think you will find that it is only exceptional for them to mention the number of animals. I think I am right there.

5486. (*Sir William Church.*) If there is any point in it, I think you would have no difficulty if you desired to find it out. You can find out year by year the number of animals used?—I only used the term "reticent"; I say they are reticent. I simply mean that they do not mention the number.

5487. Still you are aware that the Home Office

requires that the number of animals used should be specified?—I wish I had the reference; I am quite sure that in the report the number of the experiments was stated, but not the number of animals used, and that they do not necessarily correspond—that in any experiment a man may use two or a dozen animals. He may require a separate certificate for each. I do not know much about the administration of the law; I have not a mind for that sort of thing, and I have not studied it.

5488. (*Dr. Gaskell.*) You mean that in the course of an experiment upon a single animal two points may be emphasised by the observer which may be printed in different parts of the paper?—No, I mean that a man may use a dozen animals to prove one thing.

5489. Then those would all be returned?—Yes, they would be stated on the certificate, but not as different experiments, I think, although they may require different certificates.

5490. (*Dr. Wilson.*) The intention of the Act is that each animal represents an experiment?—Yes. For example, this is only one experiment I took from the Journal of Physiology on the parathyroid glands. The object of the experiment was to prove a certain thing about the parathyroid glands, but the experimenter used a number of animals to prove the same thing.

5491. (*Sir William Church.*) And he tells you the number that he used?—I think you will find that he does not tell you that.

(*Dr. Wilson.*) But in his return to the Home Secretary if the experiments were carried out in this country he would be bound to state it.

5492. (*Sir William Church.*) And they are not only here, but they are numbered. You will find six on monkeys, seven on cats, nine on dogs?—Is that in the Parathyroid Gland Paper?

5493. Yes it is in Vincent and Jolly's paper?—You will find that it is not in all; it is not the rule.

(*Dr. Gaskell.*) It states two on prairie wolves, two on badgers, two on rats—they are all mentioned.

5494. (*Sir William Church.*) I will not labour that point, but I think you are in error there. I should be much obliged if you would give me the reference to the carefully collected statistics compiled by the authorities of cancer hospitals and cancer wards, to which you refer in your Open Letter as to the question of the heredity of cancer, which you say they have disproved?—You will find those in Dr. Herbert Snow's book on "Cancer." I consider him, and I think he is generally considered, to be one at least of the greatest authorities upon cancer. And you will find that in his book he gives a number of tables, in which he shows that, although cancer in a parent may predispose to cancer, it does not do so more than diabetes or any other disease.

5495. I only wanted for my own information to know where the statistics were. I do not know that I should put perhaps the same weight upon them as some might. Then you said in your letter to Lord Selby that you had made a mistake with regard to what the Cancer Research Fund had stated?—Yes, a mistake of which I am thoroughly ashamed.

5496. I do not want to ask you anything about it, except why you say in your Open Letter "It was not worth subjecting these hundred thousand wretched little creatures to the acute and prolonged suffering of cancer in order to show that cancer is hereditary among mice." What was your reason for stating that these hundred thousand little creatures were subjected to the prolonged suffering of cancer? Have you seen any of these mice that have had cancer communicated to them?—No; but if I saw a mouse with cancer I should find it a very difficult thing to understand whether the mouse was suffering pain or not. I do not think it is possible for any human being to say from the expression or behaviour of a mouse whether, relatively to itself, it is suffering severe pain.

5497. Is cancer always a very painful affection in man?—Not always.

5498. Under what circumstances, can you tell me, is it either painful or not painful?—It generally is painful through pressure on some nerve or nerve filament.

5499. That is not the only way in which it is painful. There are other cancers besides those that are painful from pressure on the nerves, are there not?—Yes, there are, certainly.

5500. But you have not seen any of these mice?—No.

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5501. And you have not perhaps inquired from anybody who is in the habit of seeing them?—No; but I take it as a matter of fact from my clinical experience that if those tumours produced in mice were cancer, then a certain number of them, at all events, would be extremely painful; and if they were not painful, then I think it is an argument that the mouse tumour is not cancer.

5502. That may be said, but, of course, you were not aware when you wrote that that these mice had not external cancers; that if by any chance such a thing takes place, the mouse is killed and not kept alive, and you were not aware that others do not appear to suffer. I will not, of course, say that they do not, because we cannot tell, but that they do not appear to suffer?—I think I will give you a real evidence of that. I once heard a very joyous note of a bird, and went outside into the garden, and found a young bird in the mouth of my bulldog. Well, the bird had no other but a joyous note. The young thing did not know how to express pain. I think that is a very good indication that we cannot judge of the pain of an animal.

5503. I am quite satisfied that you wrote that because you thought —?—I believed.

5504. You honestly thought that it was the case, but you had no knowledge. Then you go on to talk of the practice of bleeding and other fallacies. What do you mean by that? You say: "The antitoxin of diphtheria has long been the trump card of the upholders of vivisection, although it has not yet survived so long as did the practice of bleeding, and of the hundred and one other fallacies which medicine (with honest zeal, no doubt) has upheld"?—Do you think I need to bring evidence to show that medicine has made many mistakes?

5505. You merely meant that we do not bleed as much as we used to do; but you surely believe in bleeding as a remedial measure?—I mean that bleeding is not practised to the extent that it used to be practised.

5506. How have we arrived at a better method?—I do not think the medical profession arrived at it. I think the public grew tired of our bleedings, and repudiated them, and I think that the public will do the same thing with our sera and vivisections.

5507. I suppose you know that it is still the practice?—To a certain extent; but people are not bled for every small and trifling ailment as they used to be bled, habitually and periodically.

5508. I gather from what you said to Lord Selby that you believe that there is a remedial value in some toxins?—I do not believe in them at all.

5509. You do not think that the antitoxin has any remedial value in diphtheria?—No, certainly I do not.

5510. Have you ever had an opportunity, as I have, of watching a number of cases of diphtheria and seeing those serious cases before the use of the antitoxin and then subsequently?—I think that antitoxin treatment is what is known in medicine as an abortive treatment, like, for example, the abortive treatment of gout by cold applications; or, as when a person is suffering from catarrh, the use of medicated snuff—that is an abortive treatment. It stops the catarrhal process in the nasal mucous membrane, and drives it on to the stomach and liver. It violently changes the normal course of disease.

5511. I do not want to go into such peculiar things as that?—But antitoxin is an example of abortive treatment.

5512. But you do not admit that it has good results in the way of lowering the mortality?—The mortality, on the contrary, has increased.

5513. I do not want to go into the statistics generally. I suppose when you were practising as a doctor you have seen cases of diphtheria?—Yes, a great many. I have had diphtheria twice myself, and recovered from the severest attack my doctor had ever seen, without the use of antitoxin.

5514. You are a fortunate person. You have not had an opportunity, then, of watching serious cases yourself?—Not a very great many.

5515. Will you take, please, only the case of children

who require tracheotomy to keep them alive, who are *in extremis*? Do you think that the antitoxin treatment has had no benefit in those cases?—It may have had an apparent benefit. As I was saying just now, diphtheria is in many cases a simple sore throat—

5516. I beg your pardon; I am speaking now solely of cases which either when they first go to the doctor or the hospital, or during the time they are remaining in the hospital, are so bad that they are in imminent danger of dying, and tracheotomy is performed in order that they should continue to live for a short time?—What is the question you asked me?

5517. I asked you whether you say that the antitoxin treatment has had no beneficial results in such cases?—I cannot say that I have seen enough of such cases to be able to speak to that.

5518. I do not want to go into the statistics of mortality—it would take us all the afternoon to show the fallacies; but are you aware of there being any difference in the result in those tracheotomy cases since the antitoxin treatment has been used? I am speaking only of those cases where the throat has to be opened in order that life should continue?—I must say from my experience of diphtheria that it is such a delusive thing. At one moment a child may appear to be on the point of death, and an hour later it may appear to be well.

5519. But surely you do not mean to tell the Commission that when a child's throat has to be opened to keep it alive for another quarter of an hour or twenty minutes there is any doubt about its condition and that it is all right in half an hour?—I have seen it happen so.

5520. With the presence of a false membrane?—Yes. I have seen it clear up because the presence of the false membrane causes a certain amount of spasm, and it is always difficult to say what is due to the false membrane and what is due to the spasm.

5521. Is it not the case in the vast majority of spasm cases requiring tracheotomy that it is caused from the actual presence of the false membrane in the larynx or from the larynx or in the trachea?—Yes, it is, of course to a certain extent.

5522. And when you have that solid substance, do you mean to say that in half an hour the child appears quite well sometimes?—I have seen it happen. The membrane clears away—I cannot say why—by some resistance of the system, and it all clears away.

5523. You mean that you have seen it coughed up and then the child is better?—I have.

5524. But you have no explanation to give on the fact that the mortality in these tracheotomy cases has fallen by more than 20 per cent.?—I think that statistics are so deceptive that one cannot trust to them.

5525. But we have had a good many years now running, and we have a great many former years to compare the present with, and ever since the introduction of the use of antitoxin for diphtheria there has been very little variation from year to year in the number of tracheotomy cases?—Even supposing that I admitted that, I would not consider that it was a sufficient argument for injecting a number of children who might only have a mild attack of diphtheria, with a substance which causes an increased number of cases of paralysis, inflammations of joints, convulsions, albuminuria, and sudden death, all of which are admitted to be results of antitoxin.

5526. Are you aware how the antitoxin is produced?—Perfectly.

5527. Are you aware how it is standardised?—Perfectly.

5528. Then if it has no effect upon toxin in an animal, how is it that the animals by which it is standardised which receive a dose of the toxin and the antitoxin do not die, while those which do not receive a dose of the antitoxin do die? Have you any explanation of that?—I think that the results in the lower animals are so absolutely worthless that they are not evidence and that we must go by clinical results.

5529. Surely it is evidence that there is some use in the control animals in the antitoxin.

THIRTEENTH DAY.

Tuesday, 5th March 1907.

PRESENT :

Col. The Right Hon. A. M. LOCKWOOD, C.V.O., M.P. (*in the Chair.*)

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir J. McFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary.*)

The Right Hon. The Lord RAYLEIGH, O.M., P.R.S., called in ; and Examined.

5530. (*Colonel Lockwood.*) Lord Selby writes to say that he regrets that, owing to a sudden attack of illness, he is unable to be present here to-day. I must express my thanks to the witness on behalf of the Royal Commission for coming to give evidence. (*To the witness.*) Lord Rayleigh, you appear as the representative, I think, of the Royal Society?—Yes, I do.

5531. Perhaps you would kindly state the reason of your having so come?—I think the best thing I can do will be simply to read the minute of the Council under which I attend here to-day. Resolved: "That the Council nominate no representative to give evidence before the Royal Commission, but that the Commission be informed that the President will present a statement on behalf of the Council"—or rather it should have been, perhaps—"will ask leave to present a statement." Such a statement was drawn up after careful consideration and debate by the Council, and it is that which I am here to present. I think the Council thought it might perhaps give additional weight to it if it was presented by the President in person, and that it would be more respectful also to the Commission. Shall I read the statement?

5532. If you please?—"The Royal Society, from its age and the position accorded to it among scientific institutions, feels its responsibility as a guardian of the general interests of science in this country. Founded as it was for the promotion of natural knowledge, whenever from time to time legislative changes have been proposed which might seem likely to effect the advancement of that knowledge, the Society has desired to make its voice heard on behalf of scientific progress. The recent appointment of a Royal Commission on the subject of experiments on animals has been deemed by the President and Council of the Royal Society to be an occasion when they may ask to be allowed to lay before that Commission a statement of their views on the broad scientific bearings of the question. There can be no doubt that the main cause of the remarkable development of science in modern times has been the adoption of the experimental method of investigating nature. In every department of research this method has led to the most important advances, both in questions of theory and in practical applications to the useful purposes of life. From the beginning of its history the Royal Society has fostered the prosecution of experiment, and not only in physical and chemical but in biological inquiry, and its publications are full of records of the discoveries which have consequently been made. In no branches of investigation have the theoretical and practical successes of experimental work been more conspicuous in recent years than in physiology and its practical applications in medicine and surgery. In medicine, the careful and patient testing of the effect of drugs on the lower animals has not only led to an accurate knowledge, not otherwise attainable, of these effects as produced on the human body, but has greatly increased the number

of substances now available to the physician in the treatment of disease. Without this method of investigation the progress of pharmacology, in recent years so astonishing and beneficent, would be arrested, and diseases, which may in time be successfully combated, would continue their ravages unchecked. In modern surgery the application of similar experimental work has been attended with brilliant success. Most delicate and fundamental operations on the human body have been made possible by the knowledge obtained from the treatment of animals. The President and Council of the Royal Society claim that since the continued advancement of science in every department depends so largely upon the use of the experimental method, the utmost caution should be observed in any proposals for legislation whereby the prosecution of the method might be unduly limited. So much has already been gained from the application of experiments on animals, both for the progress of physiology and for the alleviation of human suffering, and so much more may be confidently expected in the future, that the President and Council trust that nothing will be done that would hamper the legitimate employment of the method. While precautions should undoubtedly be taken against improper use of experiment on living animals, it is not the province of the Society to suggest what safeguards should be adopted. It is, however, the bounden duty of the President and Council to urge that those safeguards should be so framed as not unnecessarily to interfere with that advancement of knowledge to promote which the Society exists. Such restrictions would not only cripple or arrest the growth in this country of an important branch of biological science, but in so doing would reduce the efficiency of both physician and surgeon to mitigate or cure disease. It might then become no longer possible to maintain the high position which this country has gained in researches necessary for the advancement of knowledge, and for the guidance of medical practice; and the investigators, to whose devotion and skill the progress of medical science owes so much, might be compelled to seek in foreign universities and scientific organisations the opportunities for research which they could no longer find at home. This statement is not founded on general knowledge alone. The co-operation of the Royal Society has often been sought by the Government of this country in taking measures to arrest the spread of deadly disease, and to improve the conditions of health in distant parts of the British Empire. Without the ungrudging services of physiologists and pathologists, many of whom the Society is proud to count among its Fellows, the services thus solicited could not have been given. The President and Council gladly avail themselves of this opportunity of testifying to the laborious and unselfish devotion, often in most dangerous conditions, with which the necessary experimental researches have been carried on, and to the value of these researches, not only in enlarging our biological conceptions, but in alleviating the sufferings

*The
Right Hon.
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O.M., P.R.S.*

5 March 1907.

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of mankind." That, Sir, is the statement which I am here to present on behalf of the Council.

5533. Is there any other evidence which you have ready to tender?—I think not. That is all I am authorised to state in my official capacity; and in my private capacity I should not have ventured to appear at all.

5534. I understand then that the statement which you have just read to us is not so much your private view of the subjects dealt with as it is the general opinion of the Royal Society?—Yes, adopted after careful discussion by the Council as representing the Royal Society.

5535. That was their unanimous opinion?—I think it was unanimous. There was no formal division, at any rate.

5536. And you appear before us, not as a physiologist or pathologist, knowing anything on those subjects, but as the President of the Royal Society?—Quite so. I have no special knowledge of my own in those subjects.

5537. I will draw your attention to the first two lines in Paragraph 2, of your statement: "There can be no doubt that the main course of the remarkable development of science in modern times has been the adoption of the experimental method of investigating nature." On that subject you have no doubt?—My own experience, but it is obtained in quite other branches of science, would be that practically nothing can be done without experiment.

5538. Then in Paragraph 3, you state: "In medicine the careful and patient testing of the effects of drugs on the lower animals has not only led to an accurate knowledge, not otherwise attainable, of these effects as produced on the human body, but has greatly increased the number of substances now available to the physician in the treatment of disease." That also is your belief?—It is my belief; but I do not know that my belief has any particular value.

5539. Might I ask you what is meant by this sentence in Paragraph 4, in the last two lines: "the President and Council trust that nothing will be done that would hamper the legitimate employment of the method"—alluding to experiments on living animals?—I do not think the Council intended to define what would be a legitimate and what would be an illegitimate employment of the method. That must, of course, be for the Commission to consider.

5540. I do not quite understand what the Council meant by saying that. Do they mean the creation of any new regulation?—I suppose they had in view the possible restriction of the method in some way.

5541. By further restriction?—I suppose either present or further. I do not think that was distinguished.

5542. Then there is the paragraph beginning, "It might then become no longer possible to maintain the high position which this country has gained in researches necessary for the advancement of knowledge and for the guidance of medical practice," and so on. I understand that the Society acknowledge that this country has attained a high position in research, notwithstanding the laws laid down for the protection of animals from suffering?—That would seem to be implied.

5543. Have you ever in the course of your actual personal knowledge known medical men practice experiments on themselves sooner than go through the formalities necessary to obtain leave to carry them out on living animals?—I think you could get that better from other witnesses than from myself. I have heard talk of that nature, but I could not speak to it as of my own knowledge.

5544. But you have heard that such is the case?—Yes, I have heard talk of that character.

5545. (*Sir William Collins.*) Did I correctly understand that the statement you have submitted was prepared by the Council of the Royal Society?—Yes.

5546. Was it submitted to a meeting of all the Fellows of the Royal Society?—No.

5547. May I ask the number of the Council?—The Council, I think, are about nineteen.

5548. Could you tell us the number present on that occasion?—It so happens that I was present myself on the first occasion, but not on the second. On the second occasion, when the draft I have just read was adopted, there were present Mr. Kempe, the treasurer, in the chair, and seventeen members.

5549. So that it would be a fairly full meeting of the Council?—Yes. I may say that most meetings of the Council are nearly full.

5550. What is the number of the Fellows?—The whole number of the Fellows is something like 500. Perhaps I may explain that the Council is carefully chosen, with changes every year, in order to represent in a special manner all the more important branches of science; so that it is a representative body, representing not merely one branch of science but all branches of science.

5551. Should I be right in saying that the statement maintains that physiology is no exception to the principle that science is advanced by experiment?—I should understand that, certainly.

5552. By such experiment you would include, I suppose, not merely dissection experiments on living animals, but the administration of drugs, feeding experiments, inoculation of disease, and tests of that character?—I understand so.

5553. Are any of your councillors connected with the Association for the Advancement of Medicine by Research?—I should think it is very probable that they are. I daresay Dr. Gaskell could tell you.

5554. Do you, as President of the Royal Society, have anything to do with applications for licences or certificates?—No.

5555. Do members of your Council?—Not as such.

5556. You would not wish, I understand, to go further into the question of what operations on the human body have been made possible by knowledge obtained by experiments on animals?—I think I could not usefully speak on that subject.

5557. Would you say anything further as to questions upon which the Government of the country has sought the co-operation of the Royal Society?—I do not think I could give any complete account of that; but there has been an investigation into Mediterranean fever at Malta. That is one subject, I think, with which the Royal Society has been concerned; and an investigation of sleeping sickness in Africa.

5558. While you disclaim a knowledge of physiology, some of us may have been privileged to hear you lecture on physiological optics at the Royal Institution; at any rate on that subject one I think might ask you whether you could mention any advance in physiological optics which has been made by experiments on living animals?—I am afraid I could not speak effectively; but in a general way I feel sure that experiment is the only way in which rapid advance can be made.

5559. Can you mention any particular case of advance in physiological optics which has been obtained by experiments on living animals?—I do not think my knowledge is sufficient to allow me to go into the question.

5560. The use of the ophthalmoscope was not so obtained, I suppose?—It was by experiments on living animals, but without, I suppose, doing any damage to the animal.

5561. It would not have required a certificate or licence from the Home Secretary?—I suppose not.

5562. Should I be right in thinking that the physiology of the eye and medicine and surgery based upon that have advanced as greatly as, or, perhaps, even more remarkably than, other departments of physiology, medicine and surgery?—I suppose so.

5563. (*Sir Mackenzie Chalmers.*) I suppose the Royal Society have not considered at all whether the Home Office is the best authority for dealing with a purely scientific question like this?—I do not think they have considered that question.

5564. Sir William Harcourt instituted the plan under which all applications for licences and certificates have to be submitted to a society called the Associa-

tion for the Advancement of Medicine by Research. Have you considered at all whether a committee of the Royal Society would command more public confidence than a purely professional society?—I think it would be rather difficult for the Royal Society to undertake such a duty, considering the mixed composition of the Council.

5565. It could only be done by a committee, which would bring it back probably to nearly the same result?—I suppose so.

5566. You said that generally the experimental method was necessary. We have been told by some of the witnesses here that in physiology and in medicine observation is sufficient, and that experiment is misleading. From your knowledge of other branches of science, what would you say to that?—Of course, I am speaking now merely in my private capacity. I should have thought such a contention was absurd.

5567. (*Dr. Gaskell.*) The Royal Society has been very well satisfied indeed, has it not, with the expedition in connection with sleeping sickness?—I have always heard that the work done was excellent.

5568. They look upon those reports as being very valuable, do they not?—I have always understood so, and especially, I think, the work that has been lately concluded on the Malta fever.

5569. (*Mr. Tomkinson.*) Would your Society be brought much into contact with this question upon the surgical side in regard to surgical experiments upon living animals, or almost entirely in regard to prevention of disease, and that kind of question in which you say the Government have not infrequently consulted your Society?—I can hardly say that the Society, as a whole, is brought into connection with either the one or the other; but, of course, individual Fellows are.

5570. You say at the beginning of the last paragraph of your statement: "The co-operation of the Royal Society has often been sought by the Government of this country in taking measures to arrest the spread of deadly disease, and to improve the conditions of health in distant parts of the British Empire." Would that be the most usual form in which the Society would be consulted?—I think so.

5571. In fact, as to the prevention of disease much more than the treatment of it?—I suppose questions

proposed to the Royal Society by the Government would come under that head principally. We have, of course, papers read before the Society, sometimes by distinguished surgeons, in which the results of actual operations and experiments are described.

5572. (*Sir Mackenzie Chalmers.*) I have a list now of the Association for the Advancement of Medicine by Research. I see Professor Starling is a member of it, and he is the only member of your Council who is in it. The other members of the Society are not on your Council?—No, not at the present time.

5573. (*Dr. Wilson.*) May I ask whether a considerable number of the Fellows of the Royal Society consist of bacteriologists and physiologists, and men who have attained eminence in the medical profession?—There are a considerable number of such among the Fellows.

5574. You could not say whether they constitute a majority or not?—Certainly not; they would not constitute the majority, because all branches of science are about equally represented in the Society.

5575. Is not the blue riband of science, the Fellowship of the Royal Society, more easily won by young men who take up experimental research on animals than by those in other departments of science?—I could hardly say that. The nominations to Fellowships are very carefully discussed by the Council, and of course the object is to represent all branches—not to favour one as against another.

5576. Do not you think that the scope for bringing out something new is far greater, and greater in certainty, in the field of research than in chemistry or physics?—My own field is that of chemistry and physics, and it seems to me wide enough for anyone.

5577. I suppose you would not put the physiological laboratory upon the same plane as the physical or chemical laboratory?—In what respect do you mean?

5578. Is there not far greater liability to error and to fallacies in the results?—Mistakes may easily be made in all branches of experimental inquiry.

5579. You would not think that errors were more likely to creep in in experiments on living animals than in experiments on inanimate nature?—It is a question of the experimenter taking sufficient care to avoid being misled.

Sir R. DOUGLAS POWELL, Bart., K.C.V.O., M.D., F.R.C.P., M.R.C.S., and Mr. FREDERICK TAYLOR, M.D.,

F.R.C.P., M.R.C.S., called in; and Examined.

5580. (*Colonel Lockwood.*) (*To Sir Douglas Powell.*) You are, I think, President of the Royal College of Physicians?—Yes.

5581. Physician Extraordinary to the King, Consulting Physician to the Middlesex Hospital, the Brompton Hospital, and the Ventnor Hospital?—Yes.

5582. (*To Dr. Taylor.*) You are Senior Physician at Guy's Hospital?—Yes.

5583. (*To Sir Douglas Powell.*) I think you are prepared to make a statement to the Commission?—I do not know that I have any remarks of a general kind to make which would be very interesting to the Royal Commission; Dr. Taylor and I have handed in a memorandum of the general views that we wish to place before the Commission. I would only remark that since the last Royal Commission was held, in 1876, the science of bacteriology has been almost entirely developed, and that it has, directly and indirectly, been of infinite service in the prevention of disease, in the treatment of disease and in the mitigation of suffering, and that that has been brought about almost entirely by means of experiments on living animals; at all events, it has been largely discovered, it has been developed, and it has been tested by means of those experiments. It has almost all arisen since the evidence which was brought before the last Royal Commission in 1876—in the course of the last 30 or 40 years.

5584. Did you yourself give evidence before the last Royal Commission?—No, I did not. In that memorandum we allude very briefly to the well-known observations of Lord Lister and Pasteur, and to the infinite results which have been obtained from those observations in lessening human and animal suffering. Then we

allude to the important influence which bacteriological observations and discoveries have exercised on the public health and in the prevention of disease. Then we refer to some particular instances in which the results of bacteriological investigations have led to the treatment of certain diseases, and more particularly we mention diphtheria as an example; and we allude to the important means of standardising certain valuable remedies, such as the antitoxin of diphtheria, which can only, of course, be done by means of experiments on animals. We also refer to other specific instances, such as anthrax and swine fever particularly, amongst the diseases of animals which have been greatly mitigated by these discoveries; and the nature and treatment of cretinism and myxedema amongst the human diseases, which have been solely, one may say, established through experiments on animals by Kocher and Horsley, Schäfer and Oliver, and others. Finally, we draw attention to the important observations of Lennander, which are of recent occurrence, as showing how very little sensitive are the internal organs to surgical and other manipulations—that sensitiveness is almost entirely on the surface, and that the consequent lesions to internal organs are in the human being or in the animal attended with far less suffering than the public have been led to believe. I think those are, briefly, all the points of a general kind which I need bring before the Commission.

5585. (*To Dr. Taylor.*) Have you anything to add to what Sir Douglas Powell has just told us?—I should like to say that, speaking as the representative of the Royal College of Physicians, and representing, as I suppose one does, the views of the Fellows of the College of Physicians and of physicians in general

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practice, practising physicians that is to say—not those who are necessarily researching or making experiments themselves, but only speaking on behalf of those who have to treat disease, who have to see disease and deal with it—I may say that those of us who are constantly thinking over these problems of disease, how it is to be recognised and how to be treated, believe that it is absolutely necessary that constant research and constant experimentation and observation should be made in order to get at the problems of disease, which are extremely deep and extremely complex, and, one might say, get deeper and deeper almost every day. And while we are not making those observations and researches ourselves, I mean to say so far as experimentation on animals is concerned, we feel very deeply the absolute necessity of such observations being made in order to provide us with the means of dealing with disease in an efficient way. Much, of course, can be learnt by observation; that is what we are struggling with every day in our clinical observations in hospitals and amongst our private patients; but it must be helped by experimentation. And the same is true with regard to drugs: if new drugs are introduced they must be tested. The vegetable kingdom has been largely exploited for the purpose of obtaining means for treating disease, and the mineral world also, and the limits of these are pretty well known; and now there has been a large field of organic synthetic chemistry opened up, from which field a very large number of drugs are constantly being invented and made by advanced chemical processes. These remedies must be tested. It is perfectly true that some conception of the action of one remedy may be arrived at by a consideration of the drugs or chemicals from which it has been made; that is to say, its composition is in part known, some experimental changes are made in the composition of it by which a fresh chemical compound is obtained, and the properties or the influence which that second drug may have upon the animal body or the human body can be in part inferred from what the first chemical body does. But, of course, it must be tested on animals, it must be tested somehow or other before it is used; it must either be tested on animals or it must be tested on man. Either we must blindly experiment on ourselves and our patients, or else, as it seems to us more wisely, we must experiment on animals to find out what the effect of a drug is before we can use it on the human body. Perhaps the most striking instance of the necessity for that kind of experiment is the fact of the use of nitroglycerin. Nitroglycerin, which was first, of course, known as an explosive, does not suggest itself even to a medical man as the most likely thing to do much good in the human body; but by experimentation with nitroglycerin in extremely small quantities we know that it has certain properties, one, for instance, being that it causes dilatation of the bloodvessels; and it is largely used at the present time for that purpose; but, of course, in extremely minute doses, the dose given at first being one-hundredth part of a drop. I think it must be obvious to everyone that our ascertaining a fact of that kind must almost necessarily have been derived from experimentation upon something other than our patients; and animals, of course, had to be employed for that purpose. Then, to continue that line of consideration of the subject, it is found that most of these new drugs which are employed influence what we call symptoms—that is to say, they may remove pain, or they may depress the temperature, or they may cause sleep, or they may act upon the muscular energy, or they may act as anaesthetics. Drugs influencing the body in respect of those qualities, which have been largely derived of recent years from this synthetic chemistry, have been tested on animals almost invariably. Those drugs at the present time are not found to be of any material influence in checking the infective processes which are a large source of disease and death amongst human beings; they do not touch the duration of fever, the duration of tubercle, the duration of leprosy, or of plague, or any of those diseases with which we are most intimately and necessarily concerned; and when one comes to find what will do any good in those diseases, the experience of the last thirty years, as Sir Douglas Powell said, since the last Royal Commission, has shown that some good results have been got from bacteriology. I do not know that I need go further on that point than to say that the study of bacteriology is practically impossible without animal experimentation. Animal experimentation is practically the basis of bacteriology, and there does not seem to be any possibility that that study can be continued without experimentation being carried on in

that way; and to say what bacteriology has done, or what bacteriology is doing, or what bacteriology is hoping to do in the future, over many years yet may be, is simply to repeat what we have said in our memorandum. Vaccination with respect to small-pox is only, after all, the pioneer of bacteriology. Diphtheria, tetanus, tubercle, typhoid, Malta fever, snake poison, yellow fever, the infections of pneumococcus, streptococcus, staphylococcus (these last two being form of septicemia), have all been influenced materially by bacteriology, and there are many hopes indeed that they will continue to be influenced by bacteriology and its necessary attendant, vivisection of some kind—that is, on either a human being or animal. And, of course, to attempt to stop that process of observation does not seem a very profitable effort to make. I do not know that I have anything more to say at the present time, unless it is to enlarge upon some of the particular diseases that have been mentioned. I daresay that has been done already by some of those who have given evidence.

5586. Pray give any evidence you wish to do, regardless of former witnesses?—It would be only recalling in more or less detail the facts which have been published, of course, in many circumstances and in many ways, of diphtheria and typhoid fever and tubercle, and so forth—that is to say, the facts we know with regard to improvements in treatment and in diagnosis, and in the prevention of those various diseases.

5587. Have you read the evidence before this Commission which has been published?—I have not read it through; I have read some of it.

5588. Then is that all you wish to say on this subject?—I should like to mention a fact. I do not know how far it is really publicly or well known; but it is stated in our memorandum, namely, the considerable decrease in mortality from diphtheria since the use of the antitoxin treatment.

5589. You have stated that in paragraph 7?—Yes, we have. I have a number of figures here, which are taken from the Metropolitan Asylums Board's Report.

5590. You would like to put those in?—Yes, giving the mortality, for instance, of diphtheria amongst the cases received at the Metropolitan Asylums Board Hospital. In 1890 the mortality was 33 per cent. There were 942 patients admitted, of whom 316 died, giving a mortality of 33·5 per cent.; in the next year the mortality was 30·26 per cent.; the next year 29; the next year, 1893, it was 30 per cent. In the latter end of that year, 1893, the antitoxin treatment was introduced; and in the next year the mortality was 28 per cent.; the next year, 1895, it was 22 per cent. (I am not giving the decimal points.) In 1896 it was 21 per cent; and then the succeeding figures are 17·4, 15·1, 13·6, 12·5, 11·14, 11·13, 10, 10 per cent.—that is in the year 1904, and in 1905, 8·3 per cent.

5591. (Sir William Collins.) What was the total number admitted in those later years?—In the later years: 4,148 in 1905; in 1904, 4,687; the year before that, 5,072; the year before that, 6,520; over 7,000 in each of the two years before that; 8,673 in 1899; 6,000 in 1898; and, going back, 5,000, 4,000, and so on.

5592. (Colonel Lockwood.) That will be sufficient, I think?—In the first year, 1890, there were 942 cases, and then the numbers rose rather rapidly until the maximum admissions were 8,673 in 1899; and then the admissions gradually diminished to 4,000 again.

5593. You wish us to understand that you attribute that decrease to the use of the antitoxin?—I attribute the improvement in the percentage of mortality to that. The admissions to hospital do not count absolutely; they are dependent upon entirely different conditions; but the improvement is as between 30 per cent. and 8 per cent.; and I think it is the feeling of all of us who have seen these cases and have had to do with them, that the improvement in the treatment of diphtheria is very great indeed. Especially in that form of laryngeal diphtheria which attacks the larynx, so that patients run the risk of being strangled in the early stages of the disease, the chances of recovery are much greater since the antitoxin treatment has been used; in the laryngeal cases in which obstruction and death from asphyxia are liable to take place, more recover without the necessity of having the operation of tracheotomy, and those who have tracheotomy recover in a greater number than they used to do. Before that time the fatality in those cases was so great that there were some physicians and surgeons who felt it was almost useless to operate by tracheotomy because the cer-

tainty of death seemed so great at that time. Certainly our position with regard to that is very different at the present time.

5594. Is there anything more you wish to add?—A point has struck me with regard to tubercle and tuberculin. That is a subject, of course, which one may say is still being worked upon—the influence of tuberculin in the treatment of tubercle. Some years ago, in 1891, the tuberculin treatment was introduced by Professor Koch. The tuberculin treatment was a method of injecting practically the toxin of tubercle into the human body, when it was found that if a patient had already tubercle this injection of tuberculin toxins produced what was called a reaction—that is to say, it made the patient febrile, ill in fact. It was suggested and anticipated by Koch then that that illness would set up certain changes round the tubercle in the body, and would lead to the destruction of the tubercle. That proposition made in 1891 was taken up in a most sanguine way by the public and by the profession, and one may say that a vast experiment, a vivisectional experiment, was made on the whole civilised world; that is to say, in Western Europe and in America hundreds of people were injected with this tuberculin, with the hope that their tubercular lesions and their diseases would be cured. But it proved to be a vast failure so far as the treatment was concerned. At the same time, as a means of testing the presence of tubercle it was successful; and since that time, either by means of that tuberculin or some similar production obtained in a similar way, tubercle has been tested numbers of times with very fair success in numerous cases both in man and animals. And not only that, but a similar process has been employed with regard to the disease known as glanders, in which a similar production has been used, I believe with complete success, in horses and cattle. Anyone connected with veterinary surgery can give you more evidence on that point.

5595. Is that the mallein test?—Yes, the mallein test. So far as regards treatment by what is called the old tuberculin, that was a failure; but what I want to point out is that numbers of persons felt that the attempt to get this treatment successful at that time was hasty, and that there ought to have been more complete experiments on other than human beings all over the world—that is to say, on animals—before the human race was subjected to that vast experiment which was made at that time. Had Koch's statements been tested again all over the civilised world by other experimenters, other researchers and observers, upon animals, it is extremely probable that how much good was in it and how much bad was in it would have been found out before the human race, or the sick ones in the human race, submitted themselves to this very large experiment. What it seems to me to illustrate is that, as in the case of all these observations, when any persons make the first experiment, or the first observation, or the first discovery, their observations must be submitted to confirmation by other observers before any large measures are taken to submit the human race or human beings to their operation; and that if one could have had that done in the case of tuberculin originally, probably the loss of time and expense, sometimes perhaps fatal results, and certainly disappointment, might have been spared to many.

5596. Have you written any works on pathology or physiology?—I have written a work on the practice of medicine—a manual of the Practice of Medicine; but it is not what one calls a special work—it is only taking a general survey of the whole of medicine.

5597. Would you tell us anything you know about Haffkine's experiments upon the plague in India?—I have only got the records here in the figures derived from Mr. Stephen Paget's book, "Experiments on Animals." Inoculations on several prominent men in India were made in 1897 with Haffkine's bouillon. Of 8,142 persons so inoculated, it is believed only 18, or 0.2 per cent., took plague, and of these only two died. These died within twenty-four hours of inoculation—that is to say, they were probably already infected before being inoculated.

5598. That is an excerpt from Paget?—Yes.

5599. You have not studied the subject personally?—No, I have no special knowledge of it myself.

5600. Is that all you wish to say?—I think that is all I need say at present. With regard to drugs, I think you will have some evidence from Professor Cushny.

5601. (To Sir Douglas Powell.) Have you ever seen

any of these operations on living animals?—Yes, I have seen some.

5602. Lately?—No, not recently.

5603. Within how many years?—I was guilty of performing some myself a great many years ago, but I have not recently my own self performed them. I have recently made some observations in an endeavour to find an antitoxin for pneumonia; but those observations were carried out by Mr. Foulerton, my colleague, the bacteriologist.

5604. They were done under licence, I suppose, in the usual way?—Yes, of course.

5605. Have you ever seen any actual cruelty during the carrying out of those experiments?—No, I never have.

5606. And you do not believe that any cruelty, as we should understand it, is practised?—I should say emphatically that it is not.

5607. Have you ever seen any experiments in which the animal was allowed to recover from the operation, without being killed?—Not in recent times—not since the Act, certainly.

5608. Do you think that the laws as promulgated by the State at present governing these operations have been in any way nocuous to science?—No, I do not think so. I think as administered at the present time they have not interfered with the advance of science.

5609. Do you think that any fresh rules on the subject would be hurtful to science?—It would depend very much on what those rules were.

5610. Of course that is so. Would you object to forbidding the use of dogs for experiment?—I am quite satisfied, from the evidence which I have read of Professor Starling, and from lectures of Professor Starling's over which I have presided at the Royal College of Physicians some little time ago, that it would be impossible to get the results which he has obtained without the use of an animal like the dog.

5611. Probably owing to the reasons that we have had explained?—Owing to the reasons which he has so fully stated in his evidence.

5612. Are you in favour of allowing the continuance of experiments for lectures or exhibition purposes to students?—It is quite clear to me that some demonstrations of that kind are necessary in the present advanced knowledge, in order to convey that knowledge adequately to students; but I think, of course, that the restrictions laid down by the Royal Commission of 1876 must be scrupulously adhered to—I mean as regards the sparing of animals from any superfluous suffering, and as regards the status of those who have to do the experiments.

5613. You mean the animal not being allowed to recover from the anæsthetic—being killed before it recovers?—Yes.

5614. Are you of opinion that clinical observation and study have suffered by the use of experiments; do you think that students, through having an opportunity of practising on the live animal, pay less attention to clinical study?—I should say quite the contrary; I should say it rather stimulates their powers of observation upon the human being.

5615. Have you ever seen curare administered to an animal without an anæsthetic?—I do not recollect seeing it done. You mean in the course of an experiment?

5616. Yes?—No, I never have, certainly.

5617. You do not think that is so; you think an anæsthetic is always given with curare?—Certainly; you mean for the purpose of restraining reflex movement?

5618. Yes?—Yes, I think it is given for that purpose solely, and the animal is anæsthetised.

5619. Invariably?—Yes, on other grounds.

5620. (To Dr. Taylor.) I presume you generally agree with those answers given by Sir Douglas Powell?—I certainly agree.

5621. (Sir William Church.) (To Sir Douglas Powell.) With regard to the use of drugs, which you mention in the tenth paragraph of your *précis*, would it have been possible to make the same use of drugs that is now made by subcutaneous injection without experimentation on animals?—Not without the experience gained

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by experiment. I think it would be a most dangerous thing to do.

5622. The use of drugs by subcutaneous injection you consider a very valuable addition to our means of treating disease?—Certainly. I think that has arisen, too, since the last Commission, has it not?

5623. To a great extent. A good many, especially of those drugs now spoken of as soporifics, have been introduced since the passing of the first Act. Would it have been possible to make use of those without experimentation on animals?—I should say it would not be impossible; but it would be with great danger to those experimented upon, who would be the human beings.

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5624. Would anyone have known at all the dose of one of these soporifics which could be given with safety to man without experimentation first upon animals?—Certainly not.

5625. And, therefore, considering that the amount of the dose differs very greatly in many of these drugs, no one would have taken upon himself to have tried any experiment in anything like a dose that takes effect upon man?—No, I should think not.

5626. You, as President of the Royal College of Physicians, sign a large number of the applications for licences?—Yes, I sign great numbers.

5627. You do not sign them as a matter of course, do you?—No; I always scrutinise them carefully, and see that the person who asks for a certificate, if he is not known personally to myself, has credentials, so to speak, from one of the professors of his own department. I also investigate the reason for his proposed experiments, and I not infrequently refer the application back to him to render those reasons more clear before I sign.

5628. So that you would not sign an application for a licence for a man of whom you had no knowledge?—Not unless he was spoken for.

5629. That is having knowledge of him, when he is vouched for by a responsible teacher?—Yes.

5630. But without that you would not sign it?—No.

5631. (*Colonel Lockwood.*) Or for an experiment that you thought useless, you would not sign it?—It has not often happened that the experiments have been for a useless purpose; but in one or two instances I have referred the applications for a certificate back to the applicant to explain more precisely what his object was.

5632. (*Sir William Collins.*) Have you ever held a certificate yourself?—No, I never have.

5633. You say that you have signed applications for a great number of them?—Yes, a great number.

5634. Could you give us any idea of the number?—I am very much afraid I have not brought that with me. I should think since my presidentship, in the last two years, I have signed possibly a hundred or rather more; I could not quite say. I have got a memorandum of it, and could easily tell you; my secretary enters them all in a book.

5635. Perhaps you will kindly add that in correcting your proof?—Certainly.*

5636. Could you also give the number of cases in which you have declined to support the application?—I do not think I have actually declined in any case. I have referred applications back for further information, but I do not remember to have actually declined in any case.

5637. We were told that after the application for a licence has been made, reference is made to the Association for the Advancement of Medicine by Research for their opinion upon the subject?—That is so.

5638. Are you a member of that body?—Yes, I am, *ex officio*.

5639. As President of the Royal College of Physicians?—Yes; and I am in frequent communication with the Secretary of that Association. Sometimes I have referred an application to him to ascertain for me some further data about it.

5640. So that besides signing the application for a licence you would also have a voice in regard to the information afforded by the Association for the Advancement of Medicine by Research?—Yes, that is always open to me.

5641. Reference has been made, both by yourself and by Dr. Taylor to the reduction in the case mortality

of diphtheria, and I understand that you both attribute that to the introduction of the use of the diphtheritic antitoxin; I have not heard any other cause suggested?—Unquestionably. I do not think that anybody can be in practice without noticing the vast difference in the treatment of diphtheria which has been produced by the use of the antitoxin. Cases which one could scarcely deal with formerly are rendered amenable to treatment by the use of the antitoxin.

5642. Do you attribute the whole of the fall in the case mortality to the introduction of the antitoxin in the case of diphtheria?—I should practically do so, I think.

5643. (*Colonel Lockwood.*) (*To Dr. Taylor.*) Do you take the same view?—I do not know of any other cause. One must admit that sanitary conditions may have improved to a certain extent in the time; but I do not think that that can account for such an enormous fall as there has been. Moreover, I remember perfectly well that the first two cases that were treated at Guy's Hospital, which happened to be under my care, recovered at once; they were just as bad apparently as numbers of others that we had had before. Those first two cases, as I say, recovered. I do not attach too much importance to single cases; but the same impression, which is the result practically of unorganised experience—unorganised statistics if you like—is what all I believe who have had to do with antitoxin in diphtheria cases have derived from observation, namely, that the cases are not so fatal, that many more recover now, and that the cases of laryngeal complications, if they are injected at once, oftener get well without the necessity of tracheotomy, and that if they have tracheotomy they do not have the rapid extension of the disease to the lungs, which was almost always fatal from the broncho-pneumonia which used to be the result.

5644. (*Sir William Collins.*) And the appeal is largely to statistics such as you have quoted?—Yes; our unorganised statistics are confirmed by the organised statistics. The statistics are not only what have been published by the Metropolitan Asylums Board, but also those which have been published by similar bodies in other countries, such as Germany and France. (*Sir Douglas Powell.*) I should like to explain, with regard to your question, that the antitoxin as a treatment has nothing to do, of course, as you know very well, with the prevention of the disease except in the case of those who are immediately exposed to the risk. I think the occurrence of diphtheria may have been diminished by using the antitoxin for those persons who are in immediate contact with the original patient in the same house; but it is not used generally, I believe, as a general preventive.

5645. I was going to ask you whether it is a fact that, apart from the case mortality having fallen, the mortality from diphtheria in England and Wales has fallen?—Contemporaneously with the use of the antitoxin, do you mean?

5646. Yes?—I would not venture to say that, because while the case mortality has undoubtedly fallen, the general mortality of course would depend upon the total number of cases attacked.

5647. Is it or is it not a fact that as many people are at the present time dying annually from diphtheria in England and Wales as were dying 20 years ago?—I believe it is not a fact.

5648. I have before me the statistics of the Registrar-General for England and Wales, and I find, under the head of "Annual mortality per million persons living, from epidemic diseases": From diphtheria, in 1881, 121; in 1882, 152; in 1883, 158; and in 1884, 186; and now, taking similar figures for 1901 to 1905, I find in 1901, 273; in 1902, 236; in 1903, 182; in 1904, 170; and in 1905, 160?—Would you mind giving me the total number of people existing in the area affected in those periods?

5649. I am reading the rates of mortality per million persons living. I was not dealing with the number of deaths?—(*Dr. Taylor.*) Is there not a very great drop between 1901 and 1903, from 270 to 180?

5650. I was asking for information from those who are putting statistics in, as to the reason for the fact?—(*Sir Douglas Powell.*) I am only speaking as a practising

* *Sir R. Douglas Powell* subsequently wrote: "117 applications for various certificates, including 29 applications for licence. Several of these Certificates are held by the same person or by two workers jointly."

physician of the effect of this particular treatment on the cases that come before me.

5651. I am not doubting that for one moment; but, assuming the fact of the case mortality having fallen from 20 per cent. to 8 per cent., do you offer any suggestion as to the reason for the fact, according to the Registrar-General, that the number of persons dying from diphtheria per million in England and Wales from 1901 to 1905 does not appear to be less than it was from 1881 to 1885?—It would require a great deal of research to answer that question. I would like to know how many were treated with the remedy.

5652. Were any treated with the remedy between 1881 and 1885?—No, not in those times certainly.

5653. And I suppose a good many were treated with the remedy between 1901 and 1905?—Yes, a good many, I presume.

(*Dr. Taylor.*) May I offer a remark?

5654. I am anxious to get any information that I can?—The diphtheria case mortality in the last few years has fallen from 30 per cent. to between 8 and 10 per cent., but the number dying has been vastly more—

5655. But what I want to know is how the case mortality having by assumption, or by proof if you like, been so greatly reduced, you explain the fact of the large number of people still dying from diphtheria from 1901 to 1905?—Obviously the conditions for the spread of diphtheria and the production of diphtheria generally have not been met. That is no reflection upon the methods of treatment practised; the spread of the disease and the treatment of the disease are two entirely separate things; the disease spreads from the conditions and the susceptibility and the multiplication of the toxin, bacillus, or whatever it may be—perhaps insanitary conditions, and so forth—and passes from one person to another, and breaks out in the different persons to whom it passes. The treatment is a different thing from that, and it has no bearing upon it whatever. All one can submit is that there is no doubt a hope that in some future time diphtheria may be prevented as typhoid fever has been to a great extent prevented. Still, the fact that it has not been so prevented at present has to my mind no influence upon the consideration as regards the value of the treatment.

5656. What is your explanation or reason, then, for the fact that according to the Registrar-General's Return for 1901 to 1905 the death rate from diphtheria is more than it was 20 years ago?—(*Sir Douglas Powell.*) The cases must be more numerous generally.

5657. I was anxious to know whether either of you would proffer that explanation?—It must be so, obviously. Our statistics that the mortality has been reduced from 29 per cent. to 8 per cent. are based upon the statistics of the Metropolitan Asylums Board cases to which we have access, and also upon the case mortality in America, which is almost identical—it is practically identical. It is not one district that we take; we looked into the point fairly carefully, and I believe it to be a true statement of the case that the case mortality has been reduced from something between 29 and 30 per cent. down to something between 8 and 10 per cent.

5658. I am much obliged. The explanation I understand you to vouch for of the figures which I have put before you is, that the cases must have been much more numerous?—I think it must be so. That is the only explanation I can offer. I have not read the last Report of the Registrar-General; I have only just received it.

5659. Has the mode of diagnosing diphtheria been at all improved or altered of recent years?—The deaths from actual suffocation from the spread of lymph down the trachea, which used to be so terribly frequent, have almost been abolished.

5660. My question was whether the mode of diagnosis had been altered or improved?—I do not think so.

5661. Was the method of diagnosing diphtheria by the swab in practice, say, 20 years ago?—I do not think so; that I should think has been within the last 12 years.

5662. Should I be right in thinking that there has been a considerable improvement or alteration in the mode of diagnosing diphtheria in recent years?—Yes, I think so.

5663. That is to say, by the institution of the bacillary test?—Yes, that of course is fairly absolute.

5664. Have we been enabled by that means to diagnose as diphtheria cases which otherwise would have escaped diagnosis?—Yes, I think, undoubtedly.

5665. In considerable numbers?—Yes, in considerable numbers.

5666. Has there been any similar reduction of the case mortality in regard to other diseases dealt with in the Metropolitan Asylums Board Hospitals besides diphtheria?—I think the case mortality of such diseases as measles and scarlet fever has been considerably reduced.

5667. Measles, I think, are not treated by the Asylums Board?—Well, scarlet fever is. But I believe that the reduction of mortality in scarlet fever would be due to generally improved methods of treatment. I do not think it is due to any special treatment. (*Dr. Taylor.*) I have some figures with regard to scarlet fever which I extracted from the Metropolitan Asylums Board Reports. In 1890 the mortality was 7·8; in 1905 it was 3·2.

5668. What was it in 1880?—I have not got that. I do not think the Asylums Board published that.

5669. I beg your pardon; between 1874 and 1880, if you look at the figures of the Asylums Board, you will find that the case mortality of scarlet fever was between 10 and 14 or 15 per cent.?—(*Sir Douglas Powell.*) It varies very much with the severity of the epidemic.

5670. (*To Dr. Taylor.*) But since the year 1895 has there been a case mortality from scarlet fever exceeding 6 per cent.?—No, apparently not. It was 6·2 in 1894.

5671. (*To Sir Douglas Powell.*) To what do you attribute the fall in the case mortality from scarlet fever?—There has been a considerable improvement in the general treatment of scarlet fever with regard to the hygienic conditions of the patients.

5672. Has there been any use of an antitoxin for scarlet fever?—No, I do not think there has. Possibly in some cases the streptococcus antitoxin might have been used. I think that is quite possible. (*Dr. Taylor.*) Surely there has been a general improvement in the methods of treatment and nursing, much of which is dependent upon the improved knowledge obtained by bacteriological methods, of germs and their habits. That must have had an influence, and has had an influence upon nursing generally, not only in surgical cases, but in medical cases; and therefore, I think, that some of the improvement in scarlet fever may very well be attributed to that; in the same way as general improvement in the nursing and treatment of every sort must have felt the influence of the knowledge obtained by bacteriological methods and bacteriology generally.

5673. (*To Sir Douglas Powell.*) I see in the *précis* which you put in it is stated: "The recognition of the exact bacterial nature of the specific fevers has resulted in more precise and efficient measures for their prevention." May I ask which of the specific fevers we have exact bacteriological knowledge of?—It was typhoid fever which was more particularly in our minds with reference to that.

5674. Have we a knowledge of the exact bacteriological nature of scarlet fever?—I do not think so. At least I am not aware of it.

5675. Or of measles?—No, nor of measles.

5676. Or of typhus fever?—Well, that is practically extinct now, is it not? I do not think we have got much opportunity of studying it.

5677. Or of small-pox?—I do not think that has been ascertained—not with completeness as yet.

5678. Then I see a reference in the *précis* to less striking and less well-known instances of preventive and curative treatment of, for instance, snake poison. I do not think in your examination-in-chief you mentioned any curative or preventive for snake poison. Have we such?—Yes, there has been such. I am not an adept at snake poison myself, and therefore I can only speak from secondhand authority, but I am quite sure that subsequently to the researches of Sir Joseph Fayrer, and someone who was associated with him on snake poison, there has been an antitoxin discovered for snake poison, but I cannot quite tell you what it is.

5679. Can you give us any reference to any authority for the nature of the poison of the snake, and the antitoxin which can be safely used to prevent or cure it?—

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Calmette's and Professor Frazer's Observations on the Antidotal Qualities.

5680. Is there any evidence that satisfies you as to the preventive or curative power of any antitoxin against snake poison that has been discovered by experiments on animals?—Calmette says: "For instance, 0.15 mgrms. of viper venom is enough to kill in less than 12 hours 500 grms. of guinea-pig." "About 10 mgrms. of cobra venom are necessary to kill a dog of 6.50 kilogrammes weight," and so on. It is now nearly two years since the use of my anti-venomous serum was introduced in India, in Algeria, in Egypt, on the West Coast of Africa, and in America, in the West Indies, Antilles, etc., etc. It has been very often used for men and domestic animals (horses, dogs, oxen)."

5681. (*Colonel Lockwood.*) Who is it who writes this?—Calmette, in 1898. "Up to now none of those that have received an injection of serum have succumbed." "A great number of observations have been communicated to me, and not one of them refers to a case of failure."

5682. (*Sir William Collins.*) Is that for curative or prophylactic purposes?—For general treatment, I take it. This is an extract from the "British Medical Journal" of the 14th May, 1898.

5683. But you know that it is reported that there are some 30,000 or so deaths annually from snake poisoning in India. Have any steps been taken by the Government to utilise this alleged discovery as a means of curing or preventing snake-poisoning in India?—My only information is from this statement.

5684. Then you spoke of cretinism and myxedema as being instances in which experiments on animals had helped to a knowledge of the pathology and mode of treatment?—Yes.

5685. And you referred to Kocher and Horsley?—Yes.

5686. Should I be right in thinking that prior to Kocher and Horsley, Sir William Gull and Dr. Ord had written upon that subject?—Yes.

5687. Had they from clinical observation suggested the pathology of myxedema?—Yes, they have suggested it, but they had not proved it.

5688. Had Dr. Ord called attention to the wasting of the thyroid glands?—Yes, certainly.

5689. And I think he or Sir William Gull suggested the name cretinoid for the later condition in the adult?—Sir William Gull.

6690. Which is now known as myxedema?—Yes.

5691. So that at any rate in that regard clinical observation had facilitated or contributed towards our knowledge of the thyroid?—Yes; I am the last person in the world to deprecate the usefulness of clinical observation.

5692. I do not think you have mentioned that. You have mentioned Kocher and Horsley?—I mentioned before the use of remedies derived from experiments on animals. It was not my business in my memorandum to advocate the usefulness of clinical observation, or I might have written a hundred chapters on that subject.

5693. Would it be right to say that the pathology of cretinism and myxedema has been mainly established through experiments on animals?—Certainly the proof of it has been established by experiments on animals, inasmuch as Horsley, for instance, produced cretinism in monkeys I think by the removal of the thyroid; and I think it was Horsley—if it was not, it was the other gentleman associated with him, Kocher—cured cretinism by leaving a portion of the thyroid behind or by introducing a portion of the thyroid into the body; and I think it was Murray, of Newcastle, who first obtained the essence of the thyroid gland and used it for administration either subcutaneously or in doses by the mouth in the treatment of myxedema, and now the use of the thyroid extract is a well-established curative treatment of that disease.

5694. Although the relationship between disease of the thyroid gland and myxedema had been observed prior to Kocher and Horsley?—Certainly.

5695. (*Sir William Collins.*) (*To Dr. Taylor.*) In regard to tuberculin, did I rightly understand you to speak of the use of tuberculin some years ago as a vast failure?—Yes, certainly.

5696. And I think you suggested that if more ex-

perimentation on animals had been carried out perhaps some of the failure might have been obviated?—That is what I thought was exceedingly probable.

5697. Have you read Koch's book entitled "The Cure of Consumption," published in 1890?—I have not read it.

5698. He did experiment on animals, did he not, before publishing his tuberculin cure?—I should imagine so, but I am afraid I do not know. He must have done.

5699. I notice on page 8 there is an experiment upon a guinea-pig, and he goes on to say, "Here, again, is a fresh and conclusive proof of that most important rule for all experimentalists, that an experiment on an animal gives no certain indication of the result of the same experiment upon a human being"?—Quite so; but my argument would be that if a number of minds had been applied to the same problem and more experiments made, probably an inference or a view somewhat nearer the actual truth might have resulted.

5700. Is it true to say, as Koch did, then, that "phthisis in the early stages can be cured with certainty by this remedy"?—No, I should not think it was true by any means.

5701. We have had evidence given by a former witness as to quinine and salicylic acid. May I ask you as a physician whether quinine and salicylic acid are antipyretics?—Yes, in large doses.

5702. Are they used nowadays as such?—Very little. So far as I know, in English practice very little, not in the large doses required to reduce the temperature materially.

5703. (*To Sir Douglas Powell.*) Would you say that quinine and salicylic acid are used nowadays as antipyretics?—Quinine would be an antipyretic in malarial fever, of course, and salicylic acid would be an antipyretic in rheumatic fever—in both cases acting as antidotes to the malarial and rheumatic poisons respectively. I think that is quite in accordance with what Dr. Taylor has said. They are not pure antipyretics in the sense in which antipyrin is, which would pull down the temperature from any cause for a time.

5704. We were also told by a previous witness that by experiments on animals definite rules had been obtained, so I gathered, for the administration of digitalis in cases of pneumonia. Would you kindly give us your opinion as to that?—I am not aware of those experiments. I should think that the use of digitalis has been largely established by means of experiment, unquestionably; but I am not aware of any experiments which have established the usefulness of digitalis, especially in pneumonia. (*Dr. Taylor.*) Experiments on animals, I believe are used for standardising digitalis and similar drugs, but I should agree with Sir Douglas Powell. I have no experience of any special experiments on animals directed to the treatment of pneumonia by digitalis.

5705. (*Sir John McFadyean.*) (*To Sir Douglas Powell.*) I should like to ask you a question or two with regard to diphtheria. In your opinion is diphtheria a disease of which the medical profession now know the pathology pretty accurately?—Yes, I think they do.

5706. Would it be correct to say that the serious symptoms of diphtheria, and death when it occurs, are usually due to the action of the poison which is circulating in the system?—Yes.

5707. That poison is, I suppose, usually called the diphtheria toxin?—Yes, the toxin of diphtheria.

5708. It manifests its toxic effects on quite a large number of animals besides man, does it not?—Yes.

5709. Is it a fact that the effect of this poison on animals can be averted by the use of what is called the anti-diphtheritic serum?—Yes.

5710. Is there anyone, to your knowledge, who seriously contests that, provided there is a proper relationship between the dose of the serum and the size of the patient, and it is administered early enough?—I do not think that any well-informed person could deny that.

5711. So that it would be almost extraordinary if anti-diphtheritic serum were not of value in the treatment of cases of diphtheria in man?—Yes.

5712. I mean in view of the fact that without exception the serum can be used to avert the effects of the toxin in animals, and in view of the fact that death in human beings is due to the action of this toxin, it would be extraordinary if the antitoxin were not of value as a curative?—I presume that those are the grounds on which the antitoxin of diphtheria was introduced in human treatment.

5713. Have you any doubt whatever as to the great value of the anti-diphtheritic serum in the treatment of cases of diphtheria in human beings?—I have not the slightest doubt.

5714. With reference to the great fall in the case mortality of diphtheria in the Metropolitan Asylums Board's hospitals, are you perfectly satisfied that that reduction is at least mainly due to the use of anti-diphtheritic serum?—That is my opinion.

5715. You cannot think of any other cause?—I do not know of any other differences in treatment which would account for so tremendous a fall in the mortality. I do not think there has been any great alteration in treatment, with that one exception, within the last 20 years.

5716. Then, with regard to the fact that there is no great fall in the mortality from diphtheria throughout the whole country, might I ask whether you have any knowledge as to the proportion of cases of diphtheria, even roughly, which are at the present time or have in recent years been treated by the anti-diphtheritic serum?—It would be very difficult to say. I could not speak of that of my own knowledge. My impression is that in all central parts like towns the anti-diphtheritic serum would be largely used. In outlying places it might be more difficult to obtain; but it is becoming increasingly easy to obtain now, and I imagine that as time goes on the anti-diphtheritic serum is getting increasingly easy of access to practitioners in remote parts of the country.

5717. But do you think it is possible that half the cases of diphtheria in Great Britain and Ireland are still not treated by it?—I should think that is quite possible.

5718. So that that might partly account for the fact that the mortality from diphtheria has not been reduced?—I think I hinted at that in my answer to Sir William Collins; it was in my mind.

5719. With regard to the question of snake-bite and the prevention of snake-bite, some figures were read from Calmette, in which he indicated that the serum had been of great value in the cure of snake-bite in animals. I take it that that must have been serum administered to animals which were actually bitten by snakes?—Yes; they had received a dose of the snake poison—snake venom—first in the usual way, and then, either contemporaneously or very soon afterwards or a little before, they had received the antidote.

5720. I did not refer to his experiments, but to the use of his serum for the protection of animals. I understood you to read out that the serum had actually been employed?—Yes. "It has been very often used for man and the domestic animals (horses, dogs, oxen), and up to now none of those that have received an injection of serum have succumbed."

5721. That, I take it, must refer to animals which had actually been bitten by snakes?—Yes. Or into which the venom of a snake had been artificially introduced. This is only an extract from this gentleman's paper, which is in full in the "British Medical Journal" of the 14th of May, 1898. But I personally am only speaking at secondhand. I really know nothing about it.

5722. (To Dr. Taylor.) There is only one question I should like to put to you. With regard to the alleged great failure of tuberculin, I take it that that does not apply to the use of tuberculin for diagnostic purposes?—No. I thought I expressed that. I did not mean that at all at the time, because I instanced the use of mallein as being a corresponding method of diagnosis in another disease.

5723. Do you think it would be wrong to say that, at least so far as the use of tuberculin for use in the diagnosis of tuberculosis in animals is concerned, in-

stead of being a great failure, it has been an enormous success?—I believe it has, quite.

5724. And that it is calculated to confer immense benefits on the country?—I quite agree with that. I do not wish to decry tuberculin in general.

5725. (Sir Mackenzie Chalmers, to Sir Douglas Powell.) You are President of the Royal College of Physicians?—Yes.

5726. And you and Dr. Taylor appear on behalf of the College?—Yes.

5727. May I ask the membership of the College of Physicians—what body of professional opinion the College of Physicians represents?—It consists of some 350 Fellows and a larger number of members, and, I think, some 10,000 or 11,000 licentiates.

5728. Do you come here to-day as representing the Fellows or as representing the Council, or whom?—I have been invited to come here as representing the College of Physicians, and, I presume, through the College of Physicians, more or less the profession of medicine.

5729. I wanted to know how far your opinion is backed up by the profession generally?—I only speak as a physician, and I believe I represent the general opinion of the profession. I have not canvassed them.

5730. Your views, as put in this paper, have not been formally discussed by the Council?—No.

5731. So far as you know, the opinions which you have expressed to-day are the opinions of the leading members of the profession?—Yes.

5732. Can you give us the name of any leading member of the College of Physicians who holds a different opinion and thinks that animal experimentation either is wrong or leads to erroneous results?—No; I really cannot. I do not know of anyone.

5733. Among your own Council, is the opinion practically unanimous, then?—Yes.

5734. And your deliberate opinion is that experiments on animals are necessary to the successful carrying out of the practical profession of a physician?—The carrying out of the profession of a physician is largely based upon the results of experiments on animals.

5735. And the practice of medicine would be injured if experiments on animals were stopped, you would say?—Unquestionably.

5736. You sign a good many of the licences and certificates which come to the Home Office?—Yes, I do.

5737. I gather from your evidence that that is not a duty performed perfunctorily, but that you give it personal consideration?—It is a duty which I perform with all the care which I am able to give to it.

5738. It has been stated by one witness that licences and certificates for the performance of painful experiments without anaesthetics could be obtained by asking; may I ask whether you would say that?—I should say that is not true.

5739. That is what I wanted to know—so far as you are concerned?—Certainly; so far as I am concerned.

5740. There were one or two points I did not quite understand about diphtheria. May I ask which is your hospital?—I am consulting physician at Middlesex, and at the Brompton Hospital for consumption.

5741. In Middlesex, do you take diphtheria cases from time to time?—Yes.

5742. So that personally in hospital practice you have used the antitoxin yourself?—Yes.

5743. And you have watched its results?—Yes.

5744. (To Dr. Taylor.) And you, I suppose, at Guy's, take in diphtheria cases?—Yes. We take in diphtheria cases into what we call the isolation ward, with a diphtheria division. The cases are systematically injected with antitoxin directly they are recognised to be diphtheria, or even if there is a suspicion.*

5745. And since the introduction of anti-diphtheric serum, have you felt much more confidence in dealing with your cases?—Yes. They do not always recover, of course.

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* Dr. Taylor subsequently wrote: "I should like to qualify this statement by saying that the cases of diphtheria now admitted to Guy's Hospital are only those in which life is immediately endangered by laryngeal complications."

Sir 5746. It is a proved remedy, but not a cure?—Yes; exactly.
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5748. Not *vice versa*?—I should think rather the other way, as I have put it.

5749. As regards the general spread of the disease, then, I suppose it is greatly helped by improved means of communication and by massing a large number of children together in schools—the conditions of modern life favour the disease?—Yes, it is helped in that way.

5750. And then if you get an increased number of cases you get an increased mortality?—I think it is hindered also in house cases by the use of the serum as a preventive for those who are exposed to infection. I think that may diminish the cases on the other hand, but I should think probably the facilities of communication and the overcrowding—the crowding of the population—would, as you say, increase the number of cases.

5751. Do your nurses use it? Do you ever use it as a prophylactic for nurses?—Yes, I think it is used sometimes.

5752. I suppose in different epidemics of diphtheria you have different degrees of severity as in other epidemics?—Yes, the varying degrees of severity of cases is one of the fallacies you have to bear in mind in the treatment of all epidemics.

5753. But, still, that would not account for the greater fall in mortality in cases treated by the anti-diphtheritic serum?—No, I do not think so, extending over a fair number of years and in different places, in London and different parts of America.

5754. Who will be able to tell us about snake poison, do you know; will Sir Lauder Brunton give us that information?—I think Sir Lauder Brunton would.

5755. Then I need not trouble you about that.

5756. (*Sir William Collins.*) When you answered "Yes" to the question of Sir Mackenzie Chalmers that the collection of children in schools has of recent years helped the spread of diphtheria, would it not also be true to say that the medical inspection in schools now exercised has often been the means of checking the spread of the disease?—I think it is quite possible. I do not know how far medical inspection is universally carried out now.

5757. It is universal in London in the public elementary schools.

5758. (*Dr. Gaskell.*) (*To Sir Douglas Powell.*) In paragraph 11 of your *précis* you say that the use of the active principles of various ductless and other glands is a most important addition to the resources of the physician in the treatment of patients. I think the Commission would like to hear a little more about that, if you could enlarge upon it?—I mentioned the use of the essence of the thyroid gland in the treatment of myxedema; cases of myxedema which were formerly incurable are now completely held in abeyance for long periods of time by the use of the thyroid extract and completely cured—at least for a time. Sometimes if the patients discontinue the remedy they will relapse, but with the continuance of the remedy they may be regarded as cured. And as another instance, the use of the suprarenal extract has been found very valuable in certain cases of Addison's disease, which is due to a particular disease of the suprarenal body. I am not sure whether that comes within your question, but the suprarenal extract is a very valuable remedy in increasing the tension of the circulation when it is depressed and in restraining hemorrhage.

5759. The two cases you would think of specially to mention would be adrenalin and the thyroid extract. Those of course are very recent additions, are they not?—Yes.

5760. And they are due to laboratory experiments?—Yes.

5761. Then with respect to paragraph 12, Dr. Starling in his evidence said that in surgical hospitals the amount of pain in the surgical wards was very different now from what it was some time ago. That is so?—Yes.

5762. Would you recognise that the amount of pain and suffering has enormously diminished in recent years?—It has certainly diminished in recent years with the antiseptic method of surgery which has been introduced through the researches of Lister about twenty-seven years ago, I should say.

5763. You can almost say, then, that with good antiseptic treatment, even severe abdominal surgical operations recover with only a very trivial or no amount of pain?—I have seen a great number of very severe abdominal surgical operations, and I think I can very fairly say that when a patient emerges from the anæsthetic, he suffers very little pain afterwards; the internal wounds heal up with great rapidity, and the suffering is practically nil.

5764. And if it is clearly given in evidence that in physiological laboratories the same precautions are taken with respect to antiseptics and anæsthetics, even in severe operations which are sometimes done there, you may take it as quite certain that the animals would not be likely to suffer any more pain than in the case of surgical operations?—I think you may take that as quite certain.

5765. Then the only other question I wish to ask is with respect to any alteration possibly of the Act. I should like to know from both you gentlemen whether you think it advisable to alter the Act in one respect. The Act forbids, at the present moment, any experimentation being done in order that the student may attain manual dexterity; he may do it for increasing knowledge, but not for that particular increase of knowledge which is required in surgical operations. Do you think that an alteration of the law would be of benefit in that respect?—Well, that is a question of some difficulty to answer. I decidedly think that in some cases of operations it would be very desirable to rehearse those operations on animals before doing them upon the patient. I am distinctly with Professor Starling in regard to that.

5766. (*Sir Mackenzie Chalmers.*) You mean that the students should perform the operation under proper supervision of a skilled licensee?—I was for the moment thinking rather of a surgeon who has, in the course of his practice, thought out an operation which he considers an improved method of treating patients; I think that before trying that operation, unless he is perfectly certain of it, on a human subject, he should be able to try it on an animal. In the case of students, I have not thought out that subject at all thoroughly, but I think that under certain definite restrictions (it would have to be safeguarded), there also, before a student is let loose upon the public to operate, it might be very desirable that he should perform some of these operations.

5767. (*Dr. Gaskell.*) I was naturally alluding only to students in an advanced period of their medical career, not in the beginning?—Yes. Of course, such operations would be done with the same anæsthetic, and in the same careful manner, as they would be done on the human subject; and I think I am right in the belief that at some of the American hospitals such operations have been done.

5768. That is so at the John Hopkins Hospital, in Baltimore?—So I understand.

5769. (*To Dr. Taylor.*) Have you any remarks to make with respect to the acquiring of manual dexterity in that way?—I quite agree with Sir Douglas Powell.

5770. You think it might be desirable to relax the law in that direction?—I think so; I think it would be a very great benefit to the public, and the profession generally would certainly learn more quickly to operate than they do at present.

5771. (*Mr. Tomkinson.*) (*To Sir Douglas Powell.*) I understand that you are satisfied that anæsthesia not only can be, but is in every case of what would be otherwise very painful surgical operations, secured to the animal?—Certainly. I have no doubt of that whatever.

5772. Have you any doubt, such as has been expressed by at least one witness, perhaps more than one, of the possibility of keeping say a dog under

perfect anæsthesia for a length of time?—I do not think there is any special difficulty. Dogs are rather susceptible to anæsthetics, and a skilled physiologist recognising that can keep a dog under anæsthesia for any length of time, just as a human being can be kept under.

5773. There is no difficulty, you think, of keeping a dog under anæsthesia without killing it? It has been stated that the margin between insensibility and death is so narrow a one with the dog that it is difficult to keep it on the border line?—I believe it is more difficult than in the human subject; I am not speaking from my own individual knowledge, but I believe it is said to be so. At the same time, when a dog is kept under an anæsthetic, if it dies under it, that is an accident against the physiologist; but it is kept under the anæsthetic because the physiologist cannot do his experiment properly unless it is kept under the anæsthetic.

5774. That leads me to the next, and perhaps the only other question I have to ask you—that is about the use of curare. That, we understand, is always administered, and only administered, in conjunction with an anæsthetic?—Yes.

5775. But is this not a possible contingency, that a dog is put under a series of very severe operations, that curare is administered with the anæsthetic, that the anæsthetic may to some extent evaporate or cease to be operative, and yet the dog is kept without the power of movement, or of showing the pain it is suffering through sensation?—I cannot see why it should be so at all. The object of curare is to prevent those reflex movements which are unassociated with sensation, which occur in persons or animals who are nevertheless completely anæsthetised against pain. That is the only use of curare.

5776. I quite understand that that is the intention of it exactly, but may not the result of it equally be that although curare is only administered to prevent reflex movements, the dog may cease to be insensible, but that its otherwise voluntary movements are prevented by it, and therefore the dog may be suffering the agonies of operation and be unable to show it?—May I point out, in answer to that question, that it seems to me a scarcely practicable one, for this reason? Curare is very expensive, and very difficult to obtain—very difficult indeed—and I cannot conceive of any man using curare for any other purpose than to prevent reflex muscular movements which would interfere with his experiments under an anæsthetic. To substitute it for an anæsthetic would be a piece of clumsy extravagance which I cannot imagine any sensible man making use of.

5777. It is a very expensive drug, is it?—It is a very expensive drug, and a very difficult drug to obtain. There is a very small supply of it in the market.

5778. I suppose then, in consequence, comparatively little is used now?—Very little is used, indeed; it is very rarely used.

5779. (Dr. Wilson.) As you are a well-known authority on lung diseases, I wish to ask you more particularly a few questions about tuberculosis in relation to experiment upon animals. Do you believe that phthisis can only be contracted through infection, or do you think it can be generated *de novo* by conditions of environment or employment?—It can only be acquired by the introduction of the tubercle bacillus; it is a specific disease.

5780. So that you believe that the tubercle bacillus is the originating cause of the disease, the *causa causans*?—Yes.

5781. Are you aware that at the previous Royal Commission, I think Dr. Burdon-Sanderson and the late Sir Michael Foster both contended that there was no evidence to show that phthisis was a specific disease?—I do not think they held that opinion in recent years.

5782. No, not in recent years?—There is no question as to the influence of predisposing causes—the vulnerability to consumption.

5783. Then you believe, of course, it can only be spread by infection—that it can only be propagated by infection from case to case?—I would not say from case to case. I think it is propagated by generally distributed infection more than from case to case.

5784. As an ordinary infection?—Yes, by the tubercle infection.

5785. But does it not seem rather strange that if this disease is so highly infectious as is now maintained, its infectious nature was not more insisted upon by the profession before bacteriology came in?—The infectiousness of tubercle was insisted upon a great many years ago, in Italy particularly.

5786. But not in this country?—No; but for many years it has been recognised as a more or less infectious disease.

5787. Is it within your knowledge that in hospitals for consumption, such as the Brompton Hospital, there was no special incidence of the disease amongst the nurses or the staff generally before precautions were taken in dealing with it?—Yes; I am well aware of that, and that has always, among many other considerations, led me personally to attach much less importance in regard to case infection of consumption than is generally attached by many members of my profession. My belief is that the virulence of contagion of consumption is due largely to the association of dirt with that contagion. In insanitary surroundings the tubercle bacillus is associated with other micro-organisms. Just as in the case of the tetanus bacillus, when mixed with other infections, it is far more virulent, so I believe the tubercle bacillus, when mixed with streptococcal and other organisms, is definitely more contagious, and, therefore, in clean places like consumption hospitals and sanatoria, consumption is practically not infectious at all.

5788. Do you know of many instances in your practice of a husband who, although married to a wife who dies of consumption, escapes the disease, and *vice versa*?—I know of a very great many instances, and I believe it has been shown that the infection with regard to husbands and wives and wives and husbands is not more than that of the general population.

5789. (Colonel Lockwood.) What about the case of husband and wife sleeping together?—I would not approve of that, certainly. I think that would be tempting Providence.

5790. (Dr. Wilson.) But many do so?—They ought not to do so.

5791. You believe, of course, that heredity exerts a very powerful influence as a predisposing cause?—That is another question on which there is a good deal of difference of opinion in the profession. My own belief with regard to that is that heredity exercises a decided influence in diminishing the resistance to tubercular infection.

5792. So that it is a predisposing cause to that extent?—Yes, a predisposing cause.

5793. Would you think that the ravages of the disease might be greatly checked if more discretion as to health history were observed when people are on the point of contemplating marriage?—I dare say that prudence in that respect would be like prudence in every other respect; it would have its advantages.

5794. When the tubercle bacillus was discovered by Koch, was it not believed that it would be of immense assistance in the diagnosis of early cases of the disease?—It was believed so, and that is a fact.

5795. In the very early cases?—In the very early cases it is often of very great value. It is not always of conclusive value, but it is of very great value as one of the means of diagnosis.

5796. But is it not now ascertained that in the earlier cases of the disease the bacillus, as a rule, is not found by bacteriological examination of the sputum?—It depends upon the class of cases you are considering. In so-called concealed tubercle, or internal tubercle, tubercle of bones, you have not any means of ascertaining whether the bacillus is present or not until an operation. In cases of pulmonary disease, as soon as you get expectoration from the lungs, you get the presence of the tubercle in the sputum. I would say that in some few cases you get physical signs of the disease before you can find the tubercle bacilli, but in a large number of other cases you get evidence of the tubercle bacilli in the sputum before you can be quite sure of the evidence from physical signs, so that you must take the two together. It is one means, and a very important means, of making diagnosis.

5797. I am referring more to the statistics of

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cases admitted into sanatoria. I refer to the Cumberland and Westmoreland Sanatorium, where they have been carefully examining the sputum of patients bacteriologically ever since, I believe, the sanatorium was started?—Yes.

5798. There, I think, it was found that in the early cases of disease the bacillus was not discovered as a rule?—I should think that is an error. I should think that probably those cases are not cases of consumption—if that is at all general, I mean.

5799. Considerable insistence has been laid upon the value of tuberculin as a means of diagnosing in the case of tuberculosis in cattle. Do you ever use it as an aid to diagnosis in human beings now?—It is sometimes used. I should be very indisposed to use it myself.

5800. Now, with regard to the sputum, is it not believed now that there is not so much danger in the dried sputum that is wafted about in the air as in the moist particles that may be coughed up?—I think there is infinitely greater danger in the dried sputum.

5801. Than in coughing?—Yes.

5802. Has experimentation on animals assisted in discovering any method of cure for the disease of tuberculosis?—Yes; I think, in the first instance, experiments on animals were responsible for the establishment of the fact that the tubercle bacillus was at the bottom of the disease—was the real cause.

5803. I am referring more to remedies?—Then the use of the tuberculin is a rather complicated question. The use of tuberculin, as guided by the ascertainment of the opsonic index, has also been helped by experiments on animals.

5804. But I am referring more particularly to any curative, or supposed curative, remedy, such as the Marmorek serum, or any other allied preparation?—The modern use of tuberculin has been helped by experiments on animals. The Marmorek serum is based entirely on experiments on animals; it is obtained by experiments.

5805. Would you regard it as a successful treatment of the disease?—It is still under trial. I would not express an opinion about it; but I fear it is not going to prove very successful.

5806. Admitting now that sanatoria are of great value in assisting to arrest the disease in early cases, are you of opinion that so long as the question of environment and employment is left out of count, the use of these institutions will be greatly limited? Say that a young man is admitted into a sanatorium, and the disease is arrested. He may be a worker in a factory; he returns to his old employment and to his old home. Do not you think that, in that case, the disease will very likely recur?—Unquestionably. There is no doubt whatever that any person who recovers in a sanatorium should not return to the environment under which he became diseased originally.

5807. But that question has not been so strongly insisted on as it might be; in other words, there has been no help held out in that direction. Or I may put it that in the campaign against phthisis the important questions of environment and employment should, I contend, be in the front, as it were—that you are only dealing with the fringe of the question in establishing these sanatoria all over the country. Is that not so?—Of course, it is a most difficult problem. If you get a patient better or well from consumption you must endeavour to put him in the right way afterwards of keeping well. There is no doubt about it that it is a great difficulty.

5808-9. Is there not also this ulterior risk which the nation is probably running by the multiplication of these buildings all over the country, that young married people for example, or young people about to be married, when admitted into these sanatoria may and often do so far recover as to return to their usual employment when they have greater opportunities, so to speak, of imparting to their children this hereditary tendency to which you have alluded?—Still I am afraid you would not be justified in killing them off, and preventing their being married! You are bound to do the best you can for them as diseased persons. What their future may be really cannot enter into the mind of the physician. He has got to treat his patient. What the future of that patient may be after he has

got better the physician cannot entertain; he can only guide him for the best.

5810. But from the point of view of public policy, would it not be better that more money should be spent in that way in assisting patients to healthier employments and healthy surroundings than by admitting them promiscuously into sanatoria?—I think you must keep both questions in view. I do not think you can pit one against the other.

5811. Now just one or two questions about cows' milk. Do you believe that that is a considerable cause of tuberculosis amongst children?—By the last Royal Commission on that subject the conclusion arrived at was that it was a very considerable cause. Then about five years ago Professor Koch gave a lecture at a congress which was held in London, when he denied that there is any relationship between the bovine and human tuberculosis. Since then there has been another Royal Commission with regard to that special point, with which Sir William Church is very familiar; but I think the opinion of the profession all through has been that there is a certain danger of children acquiring tuberculosis from using milk which is infected with tubercle bacilli, even if it be of bovine origin. I have no doubt that the bovine tubercle gets very accustomed after a time to its human surroundings and becomes very virulent. That, I suppose, is the answer to the question.

5812. Is tuberculosis more common amongst children of the upper and middle classes than amongst those of the poorer classes?—Unquestionably it is more common amongst the children of the poorer classes.

5813. The children of the poorer classes you would imagine drink less milk?—They drink worse milk, and they are exposed to dirty surroundings.

5814. But do they not drink less milk?—I think if they drank more milk they would stand a better chance. I expressed that view to the last Royal Commission on the subject of milk supply.

5815. Is it within your knowledge that in Japan, for example, where all the mothers nurse their children, and where bovine tuberculosis is scarcely known amongst the cattle, still tuberculosis is very common amongst the children?—I do not know anything about the Japanese.

5816. (Sir William Church.) I should like to ask whether either of you gentlemen have ever met anybody who has been in the habit of constantly treating diphtheria who does not believe in the value of the antitoxin in the treatment of diphtheria?—I should say not, certainly.

5817. You have never met with such a person?—No.

5818. (To Dr. Taylor.) Have you ever met with anyone who has been in the constant habit of treating and seeing treated cases of diphtheria with the antitoxin who has not been perfectly certain of the value of it?—I have never met anybody holding any other view. I should say that all whom I have met with and talked with about diphtheria have been unanimous in seeing the desirability of using the antitoxin whenever the first signs of diphtheria were recognised.

5819. (Mr. Ram.) (To Sir Douglas Powell.) With regard to the use of tuberculin, is it used to any extent now in the treatment of human beings?—It is used in a certain number of cases. It is used in a different way from that in which it was used in Koch's time; it is used with more safeguards. When tuberculin is used now the blood is examined from time to time in order to ascertain whether the tuberculin has resulted in raising what is called the index of resistance to tubercle; that is the object of giving tuberculin. And tuberculin at the present time is not pushed to those large doses, and is not given in those repeated doses which tend to lower the index of resistance rather than to raise it. That is the difference between the two treatments. The actual value of tuberculin as a treatment of consumption is at the present time still *sub judice*. My own belief is that it is valuable in certain cases. I think I have seen it valuable in certain cases.

5820. And in such cases as you have spoken of, where it has been given in comparatively small doses, so as to increase the index, have you known of cases where it has done decided good?—Yes, I have.

5821. Have you known any cases where a complete recovery was made which was attributable to it?—The people have got well. They have been mostly people under sanatorium treatment for a certain time, and then external infections set up by cocci and other

mixed infections having been got rid of, the specific tubercle infection has been attacked by tuberculin, and my impression distinctly is with some advantage.

5822. That would be the case of tubercle in the lungs?—Yes.

5823. Have you known of any case of tubercle where no treatment other than the specific treatment through the system by tuberculin could be available, such as tubercle in the bladder?—It has been considerably used for tubercle in the bladder, and it has been used for tubercle in bones, and especially in a form of tubercle in the skin, and with very decided beneficial results.

5824. In all of those cases which you have just given in bone, bladder, and skin there have been beneficial results?—Well, it has been useful.

5825. Is there any place in England where this treatment of phthisis or tubercle by tuberculin is practised specifically or largely?—There are a good many places now. They are being used at a good many sanatoria.

5826. Are returns being obtained from those places?—Yes, they are being obtained.

5827. Is it to be hoped, then, that the extended use may give evidence either *pro* or *con* of the advantage or uselessness of tuberculin?—Yes. About three years ago, at the Royal Medical and Chirurgical Society, the subject of the use of tuberculin was fully discussed, and a certain measure of advantage seems to have accrued from it in bladder cases, in skin cases, and in lung cases, and since that time it has been and is being used in sanatoria, and I hope that in the course of a little time we shall be getting more definite reports.

5828. Then experience seems to encourage the belief, not that it is a useless treatment, but that it is a treatment which may be improved upon?—I think there is every hope of that.

5829. (*Colonel Lockwood.*) (*To Dr. Taylor.*) Is it not the case that pneumonia is very often the cause of death in a patient who has suffered from diphtheria?—Very often.

5830. In the case of the death of a patient from pneumonia who had suffered from diphtheria, how would that be classified in the Return? Would it be put down as death from diphtheria or death from pneumonia?—I think probably it would be returned in this way: Primary cause of death, diphtheria; secondary cause, pneumonia. In every blank certificate the Registrar-General requires that there should be put a primary disease and a secondary disease, assuming that a person may die of two things, and I think a great many cases would simply get entered as diphtheria. It would depend upon the state of mind of the practitioner who signed the return, or whether he actually recognised that pneumonia had occurred in the last few hours.

5831. Is it the case that there has been a discovery lately of a coccus present in the case of syphilis?—Yes. It is spirillum—that is to say, a spiral body.

5832. And are experiments being made on living animals within your knowledge on that subject?—My knowledge of the subject is not very extensive, but I believe that monkeys have been experimented upon to find fresh facts about this *spirochaeta pallida*.

5833. (*Sir William Church.*) In saying that pneumonia is frequently the cause of death in diphtheria you do not mean to say that it is a pneumonic process as distinct from a diphtheritic one?—By no means. I mean that pneumonia is an inflammation of the lungs, set up by the diphtheria toxic agents, just as laryngitis, causing obstruction of the larynx, arises from the same cause.

5834. (*Colonel Lockwood.*) Is there anything else you wish to say?—May I make one remark with regard to the last paragraph in our memorandum, 'We would, in conclusion, say, on the part of the whole medical profession, that we have no less regard and sympathy for suffering animals than others, nor any less urgent desire to spare them, so far as is compatible with the larger claims of humanity.' I do not know that the public always recognises what we, as medical men, know and feel with regard to human sufferings. We see patients day after day, patients going on month after month, suffering from diseases which we are trying to alleviate or to prevent their termination. That seems to me to involve a very large element of sympathy that we have a right to feel, I think, for humanity, as others feel for dogs and horses; and I do think that from that point of view the subject has not always been sufficiently put before the public when this matter has been discussed. Much has been made, for instance, of the subject of hydrophobia. There are not many people in England who have seen hydrophobia. I saw two cases some years ago; and anything more horrible one can scarcely conceive than to see a child of ten years of age, as I did, dying as it was for two or three days of that disease. Anyone, I think, who had any sort of feeling whatever would feel that a good many animals might have been sacrificed in the interest of saving that child's life and preventing the distress to her parents and all others connected with her. And so I feel with regard to the whole matter. It is said that there are hundreds of dogs experimented upon, but in the whole country—the whole world—there are thousands of people suffering. We see it every day of our lives, and those who have lived a few years, and have had a large experience of that kind of suffering, I think, have a right to desire to improve their means of alleviating those sufferings; and if it can only be done by means of vivisection then it is a fair question for consideration whether the practice should not still be maintained.

Sir R. Douglas Powell, Bart.,
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M.R.C.S.

5 March 1907.

FOURTEENTH DAY.

Wednesday, 6th March 1907.

PRESENT:

Col. The Right Hon. A. M. LOCKWOOD, C.V.O., M.P. (*in the Chair.*)

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir J. McFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. W. A. GASKELL, M.D., F.R.S.

Mr. J. TOMKINSON, M.P.

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary.*)

Mr. JOHN W. GRAHAM, M.A., called in; and Examined.

5835. (*Colonel Lockwood.*) You are Principal of Dalton Hall, in the Victoria University, Manchester?—I am.

5836. And you appear here as a witness on behalf of the Parliamentary Association for the Abolition of Vivisection?—Yes; and also on behalf of the Manches-

ter Anti-Vivisection Society and the Manchester Friends' Anti-Vivisection Society.

5837. Perhaps you will say, for the benefit of the Commission, what is Dalton Hall?—It is a hall of residence and tuition attached to the University of Manchester.

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Mr. J. W. 5838. And are its objects purely educational?—They
Graham, M.A. are purely educational; it is like a college at Oxford
or Cambridge with regard to the University.

6 March 1907. 5839. How many students have you?—We have 50
students.

5840. All resident?—Yes, all resident.

5841. Will you now give the evidence to the Commission of which we have already received a *précis*, adding to it anything that may have struck you since, as you go along?—After mentioning a few cases of acute suffering—not by way of largely extending the list otherwise brought before the Commission, but simply to give a concrete basis for discussion—I should wish to plead—(1) That the natural horror of ordinary people on hearing of such suffering represents an outraged moral sense which is the inheritance of long ages of evolution, and should not be violated or ignored in any scientific judgment of the situation. (2) That, though “Nature is cruel,” and often appears indifferent to pain, it must not be forgotten that over against the apparent recklessness to suffering which runs through the physical world, the moral sense of man, his sympathy, his instinct for righteousness, is also part of the kosmos, and the part by which we, as human beings, ought to be guided, to the over-ruling of every other consideration. (3) That the central track of human improvement has in general been along the line of increasing sympathy and tenderness; to sympathy is due what makes life worth living now, and upon it centres the hope of the future. Vivisection, therefore, in pursuing a collateral interest, mainly intellectual, is running counter to the most vital current in human progress. (4) That it will be, therefore, clear that everything depends on the relative value given to material and to spiritual considerations. I shall ask the Commission to believe that spiritual things are as real—one may say as concretely and significantly existent—as material things, and that suffering, violated trust, ruined life, conscious tyranny, are facts as actually disastrous as earthquake or frost or loss of property. (5) That we ought to extend the ordinary conceptions of love, kindness, and justice to animals, some of which possess intellectual and moral qualities which themselves give them a claim in any harmonious universe to an adequate response. All this is much helped by the fact of our common ancestry with them. (6) That an expert in physiology is not necessarily gifted with more than the average moral insight to begin with, and the constant exercise of torture inevitably produces in any of us corresponding callousness. A special professional morality in any calling is a moral danger. A temper of mind is produced in physiologists which easily passes from using animals to demanding human material, by which common fallacies may be avoided. The moral authorities of the world are against the torture of animals. (7) The needlessness of repeating well-known experiments for practice and for teaching purposes. (8) The relation of vivisection to Christianity. (9) The difficulties of the subject: Border-line cases; vaccination; sport; farming cruelties; difference between death and torture; our inherited habit of meat-eating; admitted difficulty of “drawing the line”; common comparisons between human and animal life. (10) That we cannot omit from the argument either past series or Continental series of experiments. Scientific attention may at any time be turned to subjects requiring equally painful treatment with those of the past. (11) Legislative proposals.

5842. What do you wish to state in reference to those different subjects?—I thought that, by way of a concrete basis for my argument, I might give you quite briefly, if you are willing to take it briefly, a short summary of a few cases of acute suffering caused by experimentation on living animals. I have the details here with me, and I have looked up the references in the journals, so that if at any time you wish me to expand them I shall be glad to do so. The first case I take is that of Dr. Rose Bradford, who cut into the ears of dogs.

5843. Are you going to enter into the cases of acute suffering, of which you speak, from personal knowledge or from reading?—I only propose to give just one or two cases, so that my argument may not be in the air. Dr. Rose Bradford cut into the ears of dogs, destroyed the tympanic plexus, scraped out the middle ear, and poured in pure carbolic.

5844. Can you give us the reference to this experi-

ment?—Yes; it is in a paper on the physiology of the gland nerves, which appeared in the “Journal of Physiology” for November, 1888, page 309. I have the full thing here if you want it. The operation was done under chloroform, but the dogs lived for weeks afterwards, in what must have been a condition of great suffering. For my next instance, Dr. Lauder Brunton slowly heated—or shall we say baked—rabbits to death, and along with Dr. Theodore Cash did the same to cats, the operation lasting from three to five hours.

5845. (Mr. Tomkinson.) What is the authority for that?—“The Practitioner,” Volume 33, page 273, and following; the year was 1884. The object of the research was on the action of digitalis with reference to the pulse. The temperature of the rabbits was raised to 111° and 113° Fahrenheit in one series of cases, and something a little less in another.

5846. You would not call that being baked to death, would you?—It is just whatever phrase you like to use. They were heated, at any rate, until they died.

5847. (Sir Mackenzie Chalmers.) Do you mean that the temperature of the rabbits was heated or the temperature of the place in which they were?—The temperature of the rabbits rose to that.

5848. (Mr. Tomkinson.) How do you know the temperature of the place in which they were confined?—It is not given in the synopsis. It was a kind of stove.

5849. (Sir Mackenzie Chalmers.) That is a rise of 2°. Do you mean that it started at 111° and rose to 113°?—No; there were a number of cases; the highest temperature recorded in one case was 111° and in another case 113°. There are quite a number of cases in the article altogether. For my next case Dr. Cecil Shaw cut into rabbits' eyes, and put in irritating jequirity seeds for small shot in order to produce prolonged irritation, purposely using dirty instruments; he kept this up for periods varying up to six months, in order to see whether the other eye would be sympathetically affected.

5850. What is the date of that experiment?—That is to be found in the “British Medical Journal” of the 18th June, 1898, page 158. That case came up before the public at the time, because Dr. Cecil Shaw did not happen to have a licence at the time, so that it happened to become rather a public affair.

5851. (Colonel Lockwood.) Do you happen to know what Dr. Shaw is?—No, I do not. His defence when he had not a licence was that his colleague who had a licence had really done the work.

5852. Who was his colleague?—I forget his name at this moment.

5853. (Dr. Gaskell.) You do not know in what laboratory this experiment was done?—I have not got any note of that.

5854. (Dr. Wilson.) I think it was in Belfast?—I think very likely it was in Belfast.

5855. (Sir Mackenzie Chalmers.) I understand that Mr. Cecil Edward Shaw is a Master of Arts, a Doctor of Medicine, and Master of Surgery, and he contributed to the “British Medical Journal” in 1888 an article on “Sympathetic Ophthalmia”?—Yes. I have here some details of the effects of snake poisoning which are to be found in the “Edinburgh Medical Journal” for 1870-71, at page 791. The volume was out of the library when I went to look for it, so that I have not actually seen it in the original journal for that reason.

5856. (Dr. Wilson.) That is before the date of the last Royal Commission?—Yes, it is. It is stated that there were 280 experiments performed by Sir Joseph Fayrer. I only mention that because I should like to go back to an even earlier fact—namely, a book which was published in England, translated into English, in 1787, by Fontana, on the “Bite of the Viper.”

5857. (Colonel Lockwood.) What will be your object in going back to 1787?—It is to show that research on snake bite has been going on on a very extensive scale for 120 years. He states in that book that he had carried out more than 6,000 experiments, and he says: “I have had more than 4,000 animals bitten, and I have employed upwards of 3,000 vipers, and I may have been deceived; my experiments may have been too few in number.” When we are balancing things I thought it well to mention a case like that. Then my next case is Dr. B. A. Watson, who wrote a book—an

American book—and I have not the original, but the "British Medical Journal" speaking of it said: "We trust that no one in our profession, or out of it, will be tempted by the fancy that these or such-like experiments are scientific or justifiable." The experiments were the dropping of 141 dogs from a height of 24 feet upon ledges of iron, having previously hopped their legs to increase their injuries.

5858. Was this done in America?—Yes; I take the liberty of mentioning an American case, though I have carefully avoided Continental cases, because the Americans are people like ourselves, quite as humane as we are, and things which are done in America might be done here if public opinion in England were as unawakened on the subject as it is in America; therefore I think that an American instance is not out of place.

5859 (*Sir William Church.*) May I ask the date of these experiments in dropping these dogs?—It was in 1890.

5860. (*Colonel Lockwood.*) It is an American experiment, and you only produce it, I understand, in order that you may read the remarks upon it in the "British Medical Journal"?—I produce it in order that I may show to what lengths a vivisector goes, even among people of our own race.

5861. (*Sir William Church.*) But you have not shown us yet that Dr. Watson is a man of science and an experimenter. We are all, of course, agreed that only properly-educated persons with proper views should do those experiments, but you have brought no evidence before us at present as to who Dr. Watson is?—He is only a medical man who is experimenting; I know nothing more about him. In the twenty-first volume of the "Journal of Physiology," at page 325, there is an account of prolonged observations on dogs who had their throats cut, and cannulae inserted, and the viscera exposed, by Leonard Hill and H. Barnard, on the effect of gravity on the circulation; and this is what is said: "It is absolutely essential that chloroform should not be administered during the periods of observation."

5862. (*Colonel Lockwood.*) In these experiments which you are quoting, were anæsthetics administered?—These are the experiments.

5863. But were those experiments done under anæsthetics?—The actual experiment was done under anæsthetics; it is the period of observation, which was an extended period, to which I am referring.

5864. (*Mr. Tomkinson.*) Does the paper state the length of the period of observation?—No, at least I have not a memorandum of it. It was quite prolonged.

5865. (*Dr. Gaskell.*) You do not mean that the animals were kept alive after the experiment?—They were kept alive during the observation period, and they had not chloroform during the time of observation.

5865a. But they had some other anæsthetic?

5866. (*Sir William Church.*) These were done under anæsthesia?—I think that is the one which had morphia. Morphia was used, but not chloroform.

5867. But morphia was used?—Yes; but it is not universally admitted that morphia is an anæsthetic.

5868. That is another question?—That is my point.

5869. But they were killed before they got rid of the morphia?—That is not my point at all. If I had wished to say that they were not killed I would have mentioned it.

5870. But you talked about prolonged observations?—There were prolonged observations.

5871. But still they were under morphia all the time?—Yes, they were under morphia, but not chloroform. Then on the 29th of December, 1906, and therefore it is quite of recent occurrence, and I am not aware that the scientific journal is even yet in England—in a paper before the American Association for the Advancement of Science in New York, John B. Watson, of the University of Chicago, told how he had tried to discover whether rats had a sixth sense unknown to man—namely, a sense of direction. To this end he put a rat in a box, from which the only outlet was by a maze, and he kept it there until it was thoroughly familiar with the intricacies of the exit. Then he removed its eyes, but it managed to get out. Next he extirpated the olfactory nerve, but the rat still threaded the maze. As the escape might

have been due to a sense of touch, Mr. Watson froze the rat's feet. Finally he covered its head completely with collodion.

5872. (*Colonel Lockwood.*) This is an American experiment?—This is another American experiment.

5873. I should like you if possible to confine your evidence to what is taking place in England, unless it is absolutely material to your evidence?—That as a matter of fact is my last case. It was read, as I mentioned, to the American Association for the Advancement of Science in New York on Saturday, 29th December last.

5874. (*Sir John McFadyean.*) Might I ask for the source of your information?—It has been mentioned a great deal in various papers.

5875. Would you give us one reference from which you got your information. I saw it in an evening paper myself, but it might not have been accurate?—I got it from an anti-vivisection paper that we have in Manchester called the "Quarterly Reminder."

5876. Did it quote its authority?—No, it only quoted this statement.

5877. (*Colonel Lockwood.*) We will take it that the authority for your statement is the "Quarterly Reminder," an anti-vivisection paper?—Yes. I do not know that there is any better authority available, because it has only just happened. If you will allow me, I should like to quote an opinion of Dr. F. Borel, who has been the head of a hospital for a long time, on the subject of anæsthetics, which is to be found in a letter he wrote to the "Pall Mall Gazette."

5878-9. Is this gentleman an English doctor?—I do not know at all; very likely he is a Frenchman. However, he wrote in the "Pall Mall Gazette" of August, 1889, as follows:—"Will you permit a vivisector, past, present and future—if it were necessary for the good of science and mankind—to tell those good people who believe seriously that the animals experimented on by M. Pasteur do not suffer, that they are deceiving themselves? My personal experience of fifteen years' practice gives me the right formally to deny the truth of that. I have vivisected birds, horses, frogs, rabbits, monkeys, and, above all, dogs; and I can affirm three things: (1) That it is nearly completely impossible to employ anæsthetics upon them so as to render them insensible. (2) That the sufferings of the animals after the experiments are so great that they are almost stupefied, showing the apathy and indifference of the martyr. And (3) that the employment of curare, far from diminishing sensibility, augments it exceedingly, moreover that the use of it necessitates tracheotomy beforehand, to make them respire artificially, because the curare totally paralyses all voluntary movement, and thus they would otherwise suffocate. Anyone who is accustomed to a laboratory, to physiology, or to pathological experimentation, knows that animals suffer when vivisected, and greatly, until they die. No, it is necessary for M. Pasteur to have living animals to support his thesis; this letter is not the place to inquire whether he is right or wrong; but I maintain, I, pathologist, and lately chief of a hospital, that he has imposed on brave men whose confidence he has won, when he pretends that these animals do not suffer. To listen to him one would say they come voluntarily to submit themselves to his experiments to procure pleasures hitherto unknown."

5880. (*Dr. Gaskell.*) Would you say whether this gentleman is a pathologist?—I do not know.

5881. Can you give us any reference as to him?—Yes, I have given the reference to the letter which he wrote in the "Pall Mall Gazette" of August, 1889.

5882. Can you give us any reference to any work that this Dr. Borel has done?—I have made no inquiry as to Dr. Borel; I know nothing about his researches.

5883. (*Colonel Lockwood.*) Will you proceed, please?—With regard to my first head, my point is that there is a tendency among those whose profession lies wholly with the physical tissues, to ignore and even to despise considerations which are derived from the purely mental horror and shock which men experience on reading of cruelty. In that connection I was very much interested in the admission, indeed in the contention of Dr. Starling, that pain vitiates physiological experiments. That is what we have always said, and I can desire no more severe condemnation of the great body of what he describes as "classical experiments" performed before anæsthetics had come into general use than that with which Dr. Starling has supplied

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us. It was in protest against these experiments that the anti-vivisection movement arose, and it is valuable to us to have our testimony confirmed in words which are in such startling contrast to those of Dr. Klein before the last Royal Commission, when he explained that he ignored the pain of his victims. I would ask Dr. Starling to extend his view of the machinery of escape, which he rightly says has an overpowering effect on an animal, to society's machinery of escape. We are no longer isolated individuals fighting our own battles; we are organised into societies which help one another, and if it were not for the co-operation that we give to one another we could hardly meet the blows of circumstance. Society therefore has a machinery of escape from things which hurt it, and that machinery of escape is in the sympathetic feelings, in the desire to rush to the rescue of anything that is hurt, which our movement represents. I therefore claim that sympathy takes a central place in human evolution, and that if physiology leads to callousness, it is attacking that upon which the stability of human intercourse is built. It is unsafe to entrust a body of specialists with what concerns the harmony of the whole of life. An experiment has not only a physiological reaction, but it has also an ethical one always, and sometimes others. I now pass to my second head. The mystery of pain of course is a thing which has always puzzled philosophers, and I do not pretend that there is a complete solution to it, although much may be said; but the one thing which to me is clear is that over against the apparent indifference of nature to pain, there has been set the heart of man, which is also part of the kosmos, and that if we ignore human sympathy for suffering, we count ourselves a blank in the scheme of things, we destroy the balance of nature, we are reverting to a more primitive stage, and we are undoing the last and highest work of creation. Now the tendency of materialist thinkers (this was very striking when Professor Starling strayed into evolutionary ethics) is to explain the developed present by the dim past; because the dawn was dim, such thinkers would have the noonday dim also. As regards my third point, I claim that sympathy is almost the central, is, at any rate, right in the central track of human development. It began to arise at the time when man became man, when he ceased to walk on four feet, and since then he has chiefly developed on the side of his brain, and but little on the side of his body. That cerebral development was slow and required in each individual a long time to mature. We therefore have the young helpless with their parents for a long time, and that created the human family, and in the human family people began to learn the earliest lessons of unselfishness and of sacrifice for a larger whole than themselves, the full sympathetic action amongst the members of a family which was necessary in the struggle for existence as it was then. And ever since that time the unit has kept on increasing in size; the development of sympathy has increased as men have found that more good could be done by trade and helpful co-operation than by slaughter and contention. At the present time that sympathy can only be safely carried up to a certain point. Herbert Spencer has pointed out that if you sympathise more than is possible under the circumstances of a given time, you weaken yourselves, you neglect your own interests, you suffer; and sympathy cannot be developed, therefore, beyond the point at which it becomes damaging. But every act of sympathy produces a certain amelioration in the surrounding circumstances. That amelioration decreases the sum of evil and increases the sum of happiness with which we are called upon to sympathise; it therefore makes it easier to exercise your feelings of sympathy more freely than you did before. That very increase of vogue produces more helpful acts, and they in turn re-act upon the faculty of sympathy; so the thing is cumulative and progressive, and we may look forward to the time when the highest pleasure of mankind will be found in altruistic acts, and, in the long ordering of the Eternal, the present conflict between self and others we may hope will pass finally away. I have troubled you with that disquisition in order to point out that right in the midst of that blessed sequence the production of artificial cruelty and suffering comes in. It closes up the pores of sympathy amongst those who are connected with it, and to a less degree in society which is conscious of it and sanctions it, and it discourages the possibility of the exercise of sympathy by the most sympathetic. I have no doubt

that many persons have had their life and strength considerably shortened by their work in the cause of anti-vivisection. This development of callousness, again, may also be progressive in the same way as the development of sympathy may be; therefore we are tampering with the central line of human development; and I am inclined to classify mankind chiefly, not by their wealth or rank, or even by their intellectual ability, which is only a one-sided matter, but centrally by the extent to which they can enter with sympathy into the feelings of other creatures, and in a kindly way can realise that we are all workers together. I think in that man becomes like God, if you will allow me the expression, who is the central fount and source of all sympathy. With regard to sympathy, I do not mean merely sympathetic feeling, because our brains are made so that we develop far more by a single action than we do by a great deal of feeling, and it is by accustoming the motor tracks in our brains to these kindly acts that they become habitual to us (I take it that that is the physiological meaning of habit); and in that way by experience for good or evil we are rapidly building up ourselves. A medical man sees much suffering, but he devotes himself to alleviating it. A physiologist with his assistants and servants is engaged in producing suffering, to which he is bound by human nature to become in time insensible and even unaware; and these experiments, as Dr. Starling insisted, need not have, and generally have not, any direct healing significance. When Professor Starling was drawn out of the field of physiology into the field of ethics—

5884. You are talking now of his evidence, I suppose?—I am. When Professor Starling was drawn out of the field of physiology into the field of ethics, he at once, I noticed, began to exhibit those symptoms of loose vague thinking which show that a man is off his native heath. There was something amateurish about his references to the evolution of conduct, which shows that it does not do even for an experimenter to become an authority on things in general. There is an extraordinary confusion between intellectual and moral development in his answer 3805, when he was asked by Sir William Collins whether that which would otherwise be a crime would be justifiable on the ground of pursuit of knowledge; the reply was that the pursuit of knowledge is for the advance of the race, and the evolution of the race is associated with the development of the highest social feelings. One might suppose that the gentleman had never read "Faust." I suppose it is the logical fallacy of the undistributed middle.

5885. I should like to keep "Faust" out of it if I could?—It is a case in which unchecked intellectual inquiry, you see, is accompanied by moral ruin. One hears of those who have "an eye well practised in nature, a spirit bounded and poor"; and we know that a race like the Japanese may advance rapidly in intellectual development and at the same time even lose in moral qualities; and we know that the Dukhobors and other heretics in Russia at the present time, who represent the purest level of conduct now in existence on this planet, are entirely illiterate. One is not advocating illiteracy—I spend my life as a teacher in developing the brain in myself and my pupils—but I think it is an elementary confusion of thought when a man in a responsible position before a Commission like this, where one does not speak thoughtlessly, asserts that an act criminally cruel in itself might in pursuit of inquiry re-act beneficially upon such social qualities as love and kindness and sacrifice. The fact is that almost every crime which besets our nature is due to the overgrowth of some legitimate faculty, to the pampering of some appetite which is necessary and healthy in moderation. The tragedy of this controversy, and I feel the tragedy of it, is this: I am a partisan, but I nevertheless do feel that two good things are in conflict—namely, truth and kindness—and that whilst the kindness may degenerate into mere foolish sentiment, the pursuit of truth also has its limits on the moral side. To pass to my fourth head, the strength of my plea, of course, undoubtedly depends upon the validity of one's faith in or perception of spiritual facts. A German physiologist, noticing the way in which his English students shrank from the cruelties of his laboratory, is reported to have said that he supposed they were afraid that God would do the same to them as they did to the animals. That, of course, would be a dim echo in the man's mind of a text about reaping what you sow, and it is a travesty of religious faith; but he touched bottom when he recognised that it is the spiritual part in

man and in the universe which forbids that violation of its laws. I believe that every act of cruelty is recorded on the fabric of the universe as truly as a rain shower records its channel in the sand. By widespread cruelty souls are being destroyed, not in any old-fashioned or impossible sense, but by the direct and immediate operations of a disease of the soul of which cowardice and treachery are the symptoms. A deadened sensibility of soul is as real a lameness as an atrophied limb, and the breaking up of the friendly compact between a man and his dog is a catastrophe as real as an earthquake—it breaks the harmonious order of nature. It is a crime which detonates through the spiritual environment like a thunderclap. Therefore much depends upon the relative weight that we give to material and spiritual considerations. You have to ask how the exchange stands between them, and we know that there are enormous losses possible on the side of callousness, as the records of Continental vivisection show. My view is that if the spirits of men are the crown of creation, a wound upon them cannot be compensated for by much knowledge of the body. Vivisection, of course, harmonises most easily with the thought that we are the blind servants of atomic forces cherishing fond delusions about immortality, virtuous only prudentially, and deceived by the glorious mirage called heroism and honour. You may measure pain against pain and forget all the rest; but it appears to me that that is ignoring the spiritual law behind creation—

5886. I do not want to limit your evidence or your right to say anything that you think has an actual bearing on the case, and I have allowed you considerable latitude in the way of reading a most interesting essay, but of course I should be glad if you would come as soon as you possibly can to the actual facts as relating to cruelty practised on animals?—I began, of course, with a number of facts. I am now dealing with what you might call the position of sympathy in the scheme of things. I am sorry to say that it has to be a little philosophical.

5887. I do not quarrel with that part of it, but I like, of course, to get as practical as I can?—I believe it is the central drift of our inquiry. I will try to be as practical as I can. My fifth argument, for instance, is a very practical one. Everyone says that we must be sympathetic to men, but why include animals? And it has been said before this Commission, if we begin to include animals, why stop at the dog or the horse and not go on to the rabbit, the frog and the mosquito? Our reply would be that if we believe in a harmonious universe, then every faculty has its proper organ and its proper response. If there were no sound there would be no ear—the existence of an ear implies the existence of sound. A dog and a horse are beings of wonderful and delightful intelligence; they are sensitive, sensible and amiable; they know the meaning of obedience and loyalty and shame; they have pricks of conscience and a sense of guilt; they love praise and are eager for approval; they wear themselves out in our service and they risk their lives in our defence. These very qualities in themselves are an all-sufficient reason why their trust and veneration should be met, otherwise the harmony of Nature is broken, there is a sort of volcanic eruption from another world, with horror and degradation. A vivisector once said, "Dogs are so much better to experiment upon than other animals—a few words and pats quiet them, and they trust you." Trust you! What is that? That is the dog's appeal out of its extremity against the man, whom he never believed to be such a devil until now. And that thing which goes on in a dog's mind is a violence against love and faith, and hostile to what I believe to be precious in the Eternal Sight. Professor Starling's attitude towards the relation of man to dogs shows extraordinary intellectual limitations. He says, at Questions 4236 and 4358, that the dog has survived because it has aided man in his struggle for existence, and as man is still continuing that struggle we have a right to continue to use the dog as it has always been used, and that that is indeed "its chief use." I thought it was generally known, even amongst people who have a mere smattering of science, that the struggle for existence which was due to biological variation leading to the survival of those who are fittest for that environment, as enunciated by Darwin and still operating amongst the animal and vegetable world, had in the case of man fallen into the background as a factor in his development and had been for long ages superseded by conscious effort, by co-operation, by use and wont, and by the mental and spiritual faculties which enabled man to

progress more quickly and with less loss of life than by the old method, which worked only by death. This loose way of speaking about evolution should no longer survive in scientific circles after the writings of John Fiske, Herbert Spencer, Mr. Bagehot, D. G. Ritchie, and, in fact, the whole modern school of sociologists. And it has this disastrous effect, that the morals of the jungle are supposed to be suitable for Gower Street; that you may now behave as if under the law of the survival of the fittest, and that the laws under which the tiger and Tasmanian devil have come into being are also the laws which still govern Christendom, whatever Christian people may pretend. The fact is that man and dog have lived together in co-operative partnership; and that is actually given as a reason why that partnership should now be broken up in torture. I notice that instead of recognising that he is violating a plainly written law, Professor Starling (there is really very little upon which a leading medical man declines, if pressed, to be an authority) tells us something about the soul, on which I am afraid he throws but little light by calling it by its Greek name. However, this old mystery of the soul is apparently cleared up when he tells us that the psyche of man (one almost feels that he had borrowed the soul for purposes of physiology by using that word) is bound up with his associational centres, and that anatomically we can therefore find, by measuring its associational centres, the extent of the soul of a dog. I think it must have been Professor Starling that Browning had in view in that poem of his about Dog Tray, who after rescuing a child from drowning plunged in again and fished up the child's doll, when he wrote:—

"And so, amid the laughter gay,
Trotted my hero off,—old Tray,—
Till somebody, prerogative
With reason, reasoned: 'Why he dived,
His brain would show us, I should say.

"John, go and catch—or, if needs be,
Purchase—that animal for me!
By vivisection, at expense
Of half-an-hour and eighteen-pence,
How brain secretes dog's soul, we'll see!"

I make no apology for quoting an ethical writer like Browning; I think a moral writer is as proper to be quoted in a moral argument as medical authorities are in a medical argument. I venture to think that inquiry into organs associated with mental and spiritual purposes is likely to be unfruitful. We obtain no higher appreciation of the power and beauty of a play by investigating the wonderful scene-shifting which is "bound up" with it; and Ruskin somewhere says that such thinkers may be found saying: "Let us analyse Shakespeare—so much carbonic acid, so much oxygen, so much silica, and you have Shakespeare entirely explained"; and in another place, speaking of this type of critic, he likens them to the woodworms in the frame of a picture by John Bellini, who, finding themselves on the paint, find it tasteful to them; and that is their criticism of John Bellini. So I think equally fallacious is the attempt to explain the soul of man or dog by measuring its associational centres. Turning now to my sixth point, I come to the question of hospitals. In 1892, even a man so humane, and a man whom we all honour so much as Lord Lister said it was "a serious thing to experiment on the lives of our fellow men, but I believe the time has now come when it may be tried." I have not got the context, and I do not wish to criticise Lord Lister for saying it, because I have not got the context and he may have said something which makes it all right; so you will kindly regard it not as a criticism upon him; but I mention it to show how in a man less humane than he is the practice of experimentation on animals is bound to lead in the end to a desire to carry the experiments to a complete solution with human beings.

5888. Do you know when Lord Lister said that?—It was in a lecture in 1892. I ought to try to find it; it is in the "British Medical Journal" of that year; but I repeat that I do not wish to make much of it.

5889. (Sir Mackenzie Chalmers.) The context may be important?—Yes, it may. You understand that.

5890. (Colonel Lockwood.) Perfectly?—I hope you will not try to make a point against me on that account, because I am anxious not to say anything unfair about him or anyone else; but if you once get into the habit of mind which regards a living creature as the *corpus*

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vile whose principal use is to give up its physiological secrets, it is only too easy to follow on to dealing with human beings if they be criminals, or lunatics, or paupers, or dying, or distant natives at a remote station; and we know that the Continent has gone in very much for that; we know that there are frightful instances in which cancer, syphilis, and small-pox have been given to women and foundling children, and that the medical profession has rightly raised a disturbance about it—that is a matter of public knowledge; but we must remember that vivisection originally came from the Continent, and unless we check the exaggerated claim on behalf of knowledge we may some time have to meet the demands of a dominant profession saying to us—just as we are now beginning to be told, that painful experiments are useless—that experiments on animals are always liable to the fallacy of arguing from animals to men (which is quite true), and that the safety of our dearest lives depends upon the sacrifice of a few of the weakest or worst of humanity. Dr. Sydney Ringer in his well-known Handbook on Therapeutics describes the painful consequences of his own administering overdoses of salicin, nitrate of sodium, and gelseminum to various hospital patients. I claim, then, that the appeal on this issue really lies away from the experimenters to men of acknowledged inspiration, to men of far-seeing moral insight, to men who see the light dawn first on a new duty. Carlyle was on our side. Browning said that no supporter of vivisection could be among the number of his friends. Tennyson wrote “The Children’s Hospital” for the sake of the cause. Oxford lost the services of Professor Ruskin when she built her vivisection laboratory. We have—I would not say the bulk of the clergy, nor even claim for them any special moral insight—but we have such names as those of Cardinal Manning, Bishop Westcott, Mr. Wicksteed, Canon Wilberforce, and the late Bishop of Manchester—they are all with us. Now, in regard to my seventh point, I notice the outspoken claim that is made by Dr. Starling, in Question 4050, to repeat experiments for practice and for teaching purposes. No doubt a certain vividness would be imparted to teaching by those experiments, but no doubt also they could very well be dispensed with, and only a very few students in a large class see them really. A professor of geology manages to get on without being able to produce an earthquake; a professor of physics is limited in his handling of thunderstorms, and the political economist is not able to produce enough bullion to enable him to give his pupils a true sense of the magnitude of the National Debt. I consider this claim a danger signal, a sign that we ought not to take the physiologist at his own valuation, that he is a one-sided man, great in his assumptions and his self-confidence, and hopelessly unable to estimate a mere moral issue. For the introduction of the torture of living animals to multitudes of young students scatters seeds of callousness much more widely than it does in the select laboratory where a professor gains fame, and ultimately money, by research. We cannot afford to de-humanise young students of the medical profession, even at the risk of their missing a certain facility in remembering their physiology, most of which it is their usual happy habit shortly to forget. There is a forcible passage in Herbert Spencer’s “Social Statics” in which he shows by numerous instances that cruelty to animals and cruelty to human being always go together, because, in fact, they are the same mental quality. In regard to my eighth point, I do not know whether it will be welcome here, or whether it is even permissible to compare this practice with the teaching of Christ. I know that I must ask indulgence for doing so, for, though Christianity is widely spread among us, its nominal sway is much more extensive than its real obedience, and I have no right to assume in all persons even a nominal adherence to its precepts. But I think I may just say that the precepts of Christ, we shall all agree, would hardly do for mottoes or footnotes for a manual of practical physiology. “Not a sparrow faileth to the ground without your Father,” even although He does go on to say, “Ye are of more value than many sparrows,” which we shall all agree with. “In all afflictions He was afflicted”; “Blessed are the merciful, for they shall obtain mercy”; and “His tender mercies are over all His works”; and the Apostle Paul found that all mysteries and all knowledge were of little value without love. Now this is not a special characteristic of Christianity alone; it occurs in all the lofty religions of the world, and it is indeed, I think, the test of their elevation. Every

great religion means the sacrifice of self for others, and this experimentation means the sacrifice of others for self. Buddha says, “Because he hath pity on every living creature, therefore is a man called holy.” A Hindoo speaker at the World’s Fair at Chicago said that in India the word “Christian” is a synonym for cruelty to animals; and the “Buddhist Ray,” speaking of our treatment of animals, says: “Christendom needs the humanising Gospel of our Lord the Buddha.” In regard to my ninth point, I fully admit the difficulty of drawing the line between the worst and the least bad of these cases, between the reckless disregard for pain of living creatures, of which we have only too much knowledge, and the very slight cases such as occur in public health laboratories. I admit that the drawing of a line is an almost impossible thing, this is only like other human questions in that respect, and if I were willing, as I should be, not to waste powder and shot over some small things which are done, yet if you were to ask me where I should stop, one knows how you can lead on from case to case in gradations of ever-increasing cruelty, therefore I do not know where to draw the line. I admit that there are borderland cases; I admit that the thing shades off in the end to what may be quite permissible, and I know that at the present time it is not easy, nor is it wise, for us to try to draw that line. But perhaps I may just go on to say that I condemn vivisection, but I condemn also many things which occur in sport, and many things which occur on farms; at the same time I believe it is right for us, for some of us at any rate, to concentrate our work on scientific cruelty for one particular reason, namely, that many of these other forms of cruelty are traditional, and have come down from our more barbarous ancestry. We all eat meat, as we are all inclined to revenge, and so on, we eat without a thought of harm, and it may or may not be right. But this is a new incursion of reaction into human life. In the very highest part of human life, namely, the intellectual side, the moral side is outraged; and that is why I think it is worth while in this matter to make a special effort to put it right, in addition to the fact that the cruelty suffered is far greater than in any other way. Sometimes the question is asked, Would you sacrifice 100 rabbits for the sake of one child? I should like to meet a question like that by actual facts, not by hypotheses. Take, for instance, the great research which occupied the vivisectional world a generation ago, that on the localisation of the functions of the brain. Hundreds of thousands of dogs, I should think, all through Europe, in the course of that inquiry suffered, yet we know that at the present time about one-third of the cases operated upon recover, and in those that do recover there is nearly always local paralysis. That is the sort of balance that one has to strike, and it is because of cases of that kind that I rather hope the Commission will not confine its attention to the immediate bacteriological work of the moment, but will consider what may be in the future the favourite object of research, being sure that should it become desirable to experiment upon the nerves in a way which required the persistence of sensation, the spirit of scientific inquiry would not hesitate to inflict whatever suffering was necessary, if allowed to do so by law or by public opinion. We are bound in treating with this subject to bear in mind the ominous answer given to Mr. Tomkinson by Professor Starling in reply to Question 4160, when he said that even without the opportunity of using anæsthetics he would justify vivisection, and would hold up “as worthy of admiration” on a pinnacle of “moral courage” those men who continued that torture. Of course, one knew all the time that that was the correct physiological attitude, and I am obliged to Dr. Starling for having the courage to state it. We know now what we have to guard men and animals against; we see that we are back to that stage of evolution where head-hunting was considered a school of virtue and moral courage, and the man most worthy of admiration was he who owned the largest number of scalps. Moral evolution appears to go in a spiral curve, and the cruelty which once might have been necessary for the sake of food is now honoured for the sake of information. Passing to my eleventh point, with regard to legislation, the societies whom I represent are all of them advocates of the total abolition of vivisection, and they take up that attitude because they doubt inspection of a thorough kind being practicable, they consider the use of anæsthetics unreliable, and they consider that regulation will be too weak to restrain a profession

consisting of such able and distinguished men, and I feel personally so much difficulty in suggesting one course more than another, that I feel it is rather for the Commission than myself to make any suggestion on other lines.

5891. I understand from your last few words that your society goes in for the total abolition of vivisection?—The societies I am connected with do.

5892. And you do not feel inclined to suggest any reforms in the present laws which licence experiments on living animals?—What I say is that I think it is rather for the Commission than for myself to face that question.

5893. You have not thought of any possible reform?—I do not wish to bring any before the Commission.

5894. That answer of yours, and the fact that you ask for the total abolition of vivisection, prevents my asking you several questions which I should like to have done as to the exclusion of dogs, for instance, from experimentation?—I am quite against the special exclusion of dogs.

5895. You have mentioned in your evidence the use of curare; can you quote to me any case in which curare has been administered alone without anæsthetic?—I think you will remember that I quoted that from Dr. Borel.

5896. You have no other case which has been brought to your personal knowledge?—I am not a medical witness.

5897. I know that; but I ask you: have you ever had any case brought to your notice in which curare was used alone without any anæsthetic?—No, I do not remember any at this moment. That, of course, has not been in my mind at all.

5898. (*Sir William Church.*) We have had a very interesting communication from you, which was very philosophical, and one which I am afraid I am unable to follow; but what are your grounds for stating that an expert in physiology is constantly exercising torture? You say under your sixth head: "An expert in physiology is not necessarily gifted with more than the average moral insight, to begin with, and the constant exercise of torture inevitably produces in any of us corresponding callousness"?—You observe the form of the sentence; it is a general statement.

5899. I wish for some distinct expression of opinion on your part, why an expert in physiology is constantly exercising torture, first of all; and then, secondly, why in an expert in physiology there is produced a corresponding callousness to pain and suffering. Those are the two things I want you to deal with?—I have given cases which show that some experts in physiology have been constantly in the exercise of torture—I do not say that every man is doing so every day, or that all experts in physiology are constantly in the exercise of torture. The amount of frequency of it is not a thing that I can estimate; I make a general statement there.

5900. But do you think that an expert in physiology has less average moral insight, to begin with?—I said that he had not necessarily more. I should not think of making an allegation of that kind against a body of men.

5901. You really have no evidence to bring before us to prove that vivisectioners are constantly in the exercise of torture?—I brought some evidence at the beginning to that effect. It is not my business, of course, to accumulate facts.

5902. Surely it is your business not to form an opinion about the value of vivisection unless you have inquired into the subject?—I have read an enormous amount of literature on the subject in the last twenty years.

5903. That leads me on to another question which I wish to ask you?—You understand that my object here has been to bring forward a limited number of facts not to unduly occupy the Commission, which, so far as they go, are valid, and then to speak on the theory of the matter.

5904. But you have no evidence with regard to increase of callousness on the part of scientific men?—What kind of evidence would you expect?

5905. I really do not know; I want to know what you have got?—One has general impressions about it.

5906. From whence do you get your knowledge of tortures committed under the present Act? We have only to deal with the present Act?—I am not entirely

limited by that consideration. The whole question, I thought was really before the Commission.

5907. But, still, we are concerned with the administration of the Act, and whether the Act requires any amendment, or, as you would say, it requires total abolition?—My instances were in all cases except one, which had a special reason, under the Act. There was one about snake bite which was earlier, but that was given with respect to a special reason.

5908. With regard to the special instances of cruelty which you have brought before us, we are not, of course, at the present moment acquainted with them; we shall get evidence from those gentlemen who performed them. You rather wish to lead the Commission to think that some of them were not done under anæsthetics at all?—I have given some that were not this morning by the confession of the experimenters.

5909. (*Dr. Gaskell.*) To which do you allude?—Every one of them was not under anæsthetics. "It is absolutely essential that chloroform should not be administered" is stated.

5910. But morphia was administered?—Yes.

5911. (*Sir William Church.*) You might have misled the Commission with regard to Mr. Hill's experiments, and another, in which you said that chloroform was not used, by which I thought you understood that an anæsthetic was not used, whereas it appears that you were aware that an anæsthetic was used?—Excuse me, I read you the whole thing. Morphia was used; but many people deny that morphia is an anæsthetic.

5912. Have you any knowledge as to whether morphia is an anæsthetic or not?—No; that is why I do not pronounce an opinion.

5913. But you have pronounced an opinion. You cited it as being a case of experiment without anæsthetics?—You have asked me for my opinion, and I have given it; but in my evidence-in-chief I just stated the bare fact.

5914. But you have not told me from whence you derived all this knowledge of the torture that vivisectioners inflict?—I have been reading the subject for twenty years, and therefore I have a large number of sources for that information, and I have consulted the actual journals for the evidence which I have brought before you.

5915. (*Colonel Lockwood.*) And those are the deductions which you draw from them?—Yes.

5916. (*Dr. Gaskell.*) These few cases?—You do not expect me to keep the Commission for three or four hours while I am bringing cases before it. I could have done so, but that is not my business.

5917. (*Sir William Church.*) Do you avail yourself, if you happen to be ill, or would you do so, of any of the advantages which have been received from experimentation on animals?—I have never availed myself of those advantages when I was ill, and I should be very glad indeed to do without the doubtful advantages which have been gained by vivisection.

5918. Would you be glad to do without the knowledge that we have gained?—You are questioning me on medical matters, on which I am not an expert. I should be glad if my evidence might be confined to the ethical points that I raised. My evidence is not medical evidence, and you are asking me for my opinion on medical matters. It would be a mere layman's opinion, and I would rather not give it; it would not be evidence of any value.

5919. I will ask, for instance, whether, if you had a severe attack of diphtheria, you would think it was wrong to receive an injection of antitoxin?—I really think that the difficulties of the subject as it exists are so great that hypothetical cases are not of value. I have replied to your question in this form: I should be willing that we should all do without the benefits, real or supposed, that have come from it.

5920. (*Colonel Lockwood.*) That is your answer?—That is my answer.

5921. (*Sir William Church.*) We have had a witness here who told us that none of the so-called serum remedies are of any value. You perhaps do not believe that they are?—You keep on asking me medical questions, on which I do not pretend to give you any valuable evidence.

5922. You cannot form a judgment?—Yes. I have a judgment, but I do not think it is right that I should be asked to give it

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Mr. J. W. Graham, M.A. 5923. I will put the question in this way: Do you with your present knowledge think it is right to let a person die of hydrophobia rather than make use of a serum which most of us honestly believe gives him a very good chance of life?—Of course, one would differ from your premises.

5924. (*Colonel Lockwood.*) That would be the best answer. You say that you differ from Sir William Church's premises?—Yes.

5925. And therefore you decline to answer any question of the kind?—Yes.

5926. (*Sir William Church.*) Then I should like to ask you: do you think that it is wrong to make use of serum in animals for their benefit?—I think I have answered all these questions already, by saying that I think that research would have been on wiser lines if it had been otherwise directed.

5927. (*Colonel Lockwood.*) That is your answer to Sir William Church?—That is my answer to these medical questions.

5928. (*Sir William Church.*) Then in fact you think that the infliction of pain for the acquirement of knowledge is wrong, but not merely for our convenience or to satisfy our appetites, because I gathered that you do not object to animals being castrated?—Excuse me; I said definitely that I did. I would have it done under an anæsthetic.

5929. I beg your pardon; I misunderstood you. But those are vivisection experiments on a much larger scale than any scientific ones, are they not?—What do you mean by a larger scale?

5930. I mean that a great many more animals are experimented on in that way?—That depends upon the period that you take. That does not touch the issue.

5931. (*Colonel Lockwood.*) You would not class those operations as vivisection?—We are dealing with experiments in the interests of science, at present.

5932. (*Sir William Church.*) I heard what you said about Professor Starling. You think that you, without any physiological knowledge, or having studied the subject at all, are a better judge of the value of experiments, especially with regard to the abolition of pain, than those who have made it the study of their lives?—What is your point?

5933. You made a great point of the fact that Professor Starling informed the Commission that one of the great objects of an experimenter on animals was to avoid pain, because pain vitiates his experiment?—I see. No, the object of the experiment was not to avoid pain; the object of the experiment was something different, but pain was avoided *en route*, I suppose?

5934. Yes?—What is your question about it?

5935. You said that Professor Starling's experiments were useless because they were not under normal conditions. I gathered you to say that in the essay which you have read to us?—No, I did not say that. I do not quite know what you are referring to.

5936. (*Colonel Lockwood.*) I think what you said was that Professor Starling in his evidence said that an experimenter purposely avoided pain because it spoils his experiment?—Yes; and I welcomed that statement, which is in such striking support of what we have always said.

5937. (*Sir William Church.*) Then if pain is avoided I suppose the experiment is of value?—That does not follow at all.

5938. (*Sir William Collins.*) Are you a teacher yourself?—Yes.

5939. What do you teach?—Mathematics and political economy, and I am a University Extension Lecturer in History.

5940. Do you hold any opinion as to the value or valuelessness of vivisection?—When?

5941. Do you wish now to give an opinion to the Commission on the value or otherwise of vivisection?—I do not wish to say more on that point than I have already said in answer to Sir William Church.

5942. But you wish us to take your evidence rather as that of an expert in ethical matters?—I did not put myself in that position, but I was asked by these societies to come and speak on that subject, because I

have written various pamphlets on the point in times gone by.

5943. You wish us to take it as your opinion that vivisection or experiment for scientific purposes on living animals is immoral?—That is my view.

5944. If experiment on living animals were conducted under anæsthetics, would it still, in your opinion, be immoral?—If you could guarantee that there was complete anæsthesia and not pain I would have no objection to the experiments; but, of course, the difficulty is to guarantee that.

5945. (*Colonel Lockwood.*) Before and after?—The whole thing. My objection is to the cruelty. If cruelty can be avoided—certainly avoided and surely avoided, I am satisfied; then I should rejoice in the experiments.

5946. (*Sir William Collins.*) May I then take it from you that if experiments on living animals could be carried out anæsthetically, no moral principle in your opinion would be violated by such experiments?—Quite so; but you will observe what I say—certainly and surely carried out anæsthetically, without any doubt.

5947. Did I rightly understand you as representing the Parliamentary Association for the Abolition of Vivisection, to advocate the introduction of a law which should absolutely forbid the practice of vivisection?—That is the view of that Society.

5948. Do you not speak on their behalf?—Well, I do, broadly; I think I said that it was in favour of the abolition of the practice.

5949. The Association is called the Parliamentary Association for the Abolition of Vivisection?—That is so.

5950. Am I right in assuming that it is their intention, therefore, to lead up to Parliamentary action?—I believe that is so.

5951. Should I be right in thinking, then, that their object and yours would be the introduction of a Bill to abolish by law the practice of vivisection?—Yes.

5952. Do you think that if such a law were passed it would reduce the number of animals utilised for vivisection?—I think it would, inasmuch as it would not then be possible legally and truthfully to describe experiments in the medical journals, and that would take away the fountain of experimentation—the source and motive of it.

5953. Do not you think that it might have the effect of driving vivisection to other countries where such a law would not be in force?—I think it would have that effect, but, of course, in diminished numbers from those which obtain now in England. Some of them would go.

5954. Might it not also lead to the practice of vivisection surreptitiously, without regulation?—It might. I believe there is no complete solution to the question. I agree to that. I think it would.

5955. If both those occurrences might take place, do you still think that the effect of such a law would be a reduction of the total number of animals utilised for vivisection?—Yes, I should think it would.

5956. Is not that rather a speculative opinion?—It is very speculative. I find similar difficulties in a scheme of regulation; that is why I have not gone into that this morning. I believe that if I had laid before you a scheme of regulation, for which I should have had no authority except as my own view, that also would have been subject to great difficulties.

5957. Has your Association, which advocates Parliamentary action with a view to the introduction of a Bill to completely forbid the practice of vivisection, contemplated the kind of penalty which should be applied to the infraction of such a law?—I am not a member of the committee of that Association, and I have no part in its action.

5958. But you are speaking as a witness on its behalf?—I am. I cannot tell you whether they have gone into that or not.

5959. Then neither you nor your Association on behalf of whom you speak have thought out the question of the penalty that should be imposed for the breach of such a law as they have in their contemplation?—I personally have not. I do not know whether they have or not.

5960. (*Sir John McFadyean.*) I think you wish this Commission to believe that it is impossible to anæsthetise dogs completely?—I have no opinion of any value upon that point, but I quoted to you the opinion of Dr. Borel.

5961. You did wish us to accept Dr. Borel's opinion?—I naturally read it with that object.

5962. Have you any personal information as to whether Dr. Borel had a large experience in attempting to anæsthetise dogs?—He says "I have" for fifteen years "vivisected birds, horses, frogs, rabbits, monkeys, and, above all, dogs."

5963. But he does not say that he anæsthetised them?—He says that from his experience and being accustomed to pathological experimentation he knows all that.

5964. But I want to ascertain whether it is certain that he said he himself administered an anæsthetic to a dog?—You would hardly expect him to vivisect them for fifteen years without, would you?

5965. I do not know. If I am to believe what is published by some anti-vivisectionists I should be inclined to believe that he might do it?—This is not published by anti-vivisectionists.

5966. But we do not know anything about Dr. Borel at all?—I do not know whether you do or do not.

5967. Does he sign himself M.D.?—The signature is not in this extract that I have.

5968. On what ground did you call him "Dr." Borel?—That is what it is headed here, "Dr. F. Borel." He is the head of a hospital, and has been for fifteen years a vivisector.

5969. You do not pretend to be able to form a first hand opinion as to whether it is possible to anæsthetise dogs or not?—I have carefully avoided giving such an opinion.

5970. Will you tell me whether if veterinary surgeons who have for operations on dogs in veterinary practice chloroformed a thousand were to give evidence to the Commission to the effect that in their opinion dogs can be anæsthetised, just like human beings, we ought in your opinion to allow that evidence to neutralise Dr. Borel's?—I should go by the weight of testimony.

5971. You have made it perfectly clear in answer to a previous question, that all your objections to vivisection would fall to the ground if you could be satisfied that it is possible to completely abolish consciousness, to abolish pain during the whole course of the experiment?—And afterwards.

5972. I do not want to ask you any more about that, but I want to ask you this: Do your objections to vivisection extend to cases in which there is a very trifling amount of pain inflicted?—I think I confessed that it was very difficult to draw the line.

5973. This Commission is expected to draw the line?—Very likely. I am glad I am not a member of it, then.

5974. You have come here to advise us?—I know.

5975. You decline to answer the question?—I think I have answered it.

5976. You have not answered it so definitely as I should like you to do. I should like a straightforward answer to the question. Assuming that useful discovery might be made by experiments on animals which involved no more pain than that inflicted by the insertion of a hypodermic needle?—Followed by no disease?

5977. I will ask you to assume, or believe, if you like, that it is followed by disease, but that the pain of the disease again is trifling?—One does not, of course, quite know how the word "trifling" is used by vivisectors. We have Professor Starling saying that the pain of starvation is trifling.

5978. I will allow you to interpret the word trifling as you like—what you would call trifling?—I do not know quite what the disease would be to which you refer as causing trifling pain.

5979. I ask you to assume that there are such diseases. You do not pretend to come here, do you, to give first hand evidence as to physiology and pathology?—No.

5980. If I ask you to believe that there are diseases which run their course in man without severe pain,

you are not going to contend that that is wrong, are you?—You see my difficulty in making any admission is that from one case you may go on to a stronger case, and a stronger case until you do not know where to stop. I do not wish to burke the thing—
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5981. I ask you to answer the question without considering the ulterior consequences of it?—But I believe practically (this is the issue I think we have come to) that if we got a law on the subject the Courts would have to determine in each particular case under the ordinary law of cruelty whether the thing was serious enough for action or not.

5982. That is not an answer to my question. I shall be quite satisfied if you say you refuse to answer it, but I would rather that you should answer it. My question is whether the infliction of a small amount of pain, a trifling amount of pain, on an animal would be justifiable if thereby we acquired useful knowledge with regard to the prevention or cure of disease?—The ambiguity of the words I think makes it best for me not to answer the question.

5983. What particular word is ambiguous?—The word trifling. I may say I would speak much more freely if we were talking together as individuals, but you see I have behind me a certain amount of opinion, and on these difficult issues—these borderland cases—one man may think one thing and another another, and I would rather not lay down anything definite about it.

5984. If you refuse to answer, I, of course, shall be satisfied; but I am going to amend the word that you said was ambiguous. You have, I suppose, sustained the prick of a needle, if "sustained" is not too strong a word?—Yes.

5985. I will ask you to assume again that useful knowledge can be obtained by experiments which involve no more pain than the insertion of a needle into your own skin?—And not followed by disease?

5986. Not followed by any pain at all?—I should be extremely willing to prick a cat or any other animal as I would prick my own finger.

5987. That was all I really wished you to say?—But we know that hydrophobia and cholera are produced in that way.

5988. But you must not give us evidence on physiology and pathology. It is only on ethics that you pretend to speak?—I have tried to avoid doing so.

5989. Then I also want to return to a subject on which you were pressed by Sir William Church, and on which it seemed to me you did not answer quite fully—that is, as to whether it is right to take advantage of the valuable methods of treatment and prevention which have been discovered by vivisection; and I want you to note, please, that that is not a pathological or physiological question. It is an ethical question?—You are quite right in asking me that question.

5990. I am going to assume that there are such valuable methods of treatment and prevention. Do you think it is morally right to take advantage of them?—I think, strictly speaking, that it is not morally right.

5991. Therefore it ought to be stopped?—We do not stop everything by law that is morally wrong.

5992. But you think it ought to be discouraged?—I have answered that question—yes.

5993. So that you think a medical man is not acting quite correctly in following those methods?—That is a matter for his own conscience.

5994. Your horror of vivisection, I gather, extends to all cases in which man inflicts pain on animals?—For scientific research.

5995. No. I mean, does it also extend to other cases in which man inflicts pain on animals?—Other cases do not come before this Commission.

5996. But I submit that I have a perfect right to ask you what your attitude is in regard to other forms of cruelty, in order to find out whether you are consistent or not?—I am against all forms of cruelty.

5997. You yourself introduced a reference to certain operations performed on animals?—Yes; I wish to meet that.

5998. Would you advise that the rearing of game for the mere purpose of killing them by methods which are

Mr. J. W. often painful, should be prohibited by law?—I am against it.
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6000. Would you advise that there should be legislation to put down field sports?—That is a very complicated question, and many things come in there which do not come in here.

6001. It is so complicated that you would rather not answer it?—It is so complicated that I have never contemplated the question.

6002. I ask you to contemplate it now, please?—But you cannot expect me to answer upon a political issue, which touches English life at many points, on the spur of the moment.

6003. By a political issue, do you mean with regard to the probability of legislation being securable?—No, I mean that every Act of legislation is an extremely responsible thing, and I do not form an opinion upon a new issue upon the spur of the moment.

6004. (*Colonel Lockwood.*) That is your answer?—That is my answer.

6005. (*Sir John McFadyean.*) You are aware that a great deal of pain is inflicted on animals in killing them for human food?—It should not be.

6006. Are you aware that a great deal of pain is inflicted?—I would do what I can to minimise it.

6007. That I take to be an affirmative answer to my question—that you are aware that a great deal of pain is inflicted upon animals in killing them for human food?—That is true.

6008. Is that avoidable?—I believe so.

6009. How?—I am not an authority on slaughterhouses, but I do know that they can be very greatly improved, and that much may be done in the way of humanising them, and that the difference between the best and the worst now is very great.

6010. I will put the question to you in this form: Would it be absolutely impossible to insist that the animals should be anaesthetised before they are killed?—I do not know. It is an interesting question, and I should be very glad if it could be done.

6011. Assuming that it is possible, would you advise that there should be legislation to secure it?—Yes.

6012. You would actually advise that all sheep and cattle that are killed for food should be anaesthetised?—If it were possible.

6013. Assuming that it were possible. At any rate, there is no doubt that the smaller animals might be treated like the larger ones. I mean that the sheep and the pig and so on might be stunned as cattle are?—Yes.

6014. Would you advise that there should be legislation to insist upon that also?—I think it would be a very good thing. You will understand, please, that our testimony is not against the death of creatures; it is against the torture of creatures.

6015. Quite so. But I merely want to know to what extent you would advise that one should go in order to render the existence of the lower animals absolutely painless?—Yes, I quite see that.

6016. Are you a vegetarian?—No, I am not—not entirely.

6017. Do you think it is quite consistent to eat animal food which is secured to you after the infliction of so much avoidable pain?—I doubt if it is quite consistent. I think consistency is an extremely difficult art in real life.

6018. We shall admit that. I expect you will give very much the same answer as regards the painful operations of castrating and spaying. I think you have already said that you would advise that those operations should only be carried out under anaesthetics?—That is my view. It is rather an ignorant view, but it is my view.

6019. As you have described it yourself as an ignorant view, I do not think I need question you any further about it?—I do not think you need, really.

6020. I merely wanted to point out that it would involve the anaesthetising of some millions of animals annually in this country?—I should not have thought that the numbers were so large.

6021. Do you know how many sheep there are in this country?—No.

6022. Do you know that about 99 per cent. of the male sheep are castrated?—No.

6023. And that the same proportion of male and female pigs are castrated or spayed?—I think I justified my special action with regard to vivisection on sundry grounds, which I need not repeat. I am against cruelty in all its forms at all times, and one admits that one goes in for different methods of preventing it in different branches of the subject.

6024. If you will pardon me for saying so, you are yourself using an ambiguous term when you speak about cruelty. That rather begs the question in many cases, because it is not always possible to settle what justifies pain, and it is impossible for man to secure a painless existence to animals?—Yes, I quite agree. I believe that the thing is full of theoretical difficulties; I admit that.

6025. I put it to you that in the months of spring and early summer in this country farm places simply, so to speak, seethe with vivisection; that male and female animals have these sensitive organs cut out of their bodies in full consciousness, and that this is done on millions of animals annually. Have you ever made any effort to reduce this animal suffering?—One can only do a few things in life, and I am not a missionary for agriculture at all.

6026. Will you take it from me that millions of animals are yearly, I will not say tortured in this way, but subjected to severe pain?—I might be willing to take it up if I had the strength and the time.

6027. Will you admit that the total amount of animal suffering inflicted in that way must be vastly in excess of what is caused by physiologists and pathologists in experimentation on animals?—I do not know enough about it to admit that.

6028. Have you studied the statistics as to the number of animals that are experimented on in this country?—By vivisection?

6029. Yes?—Yes, 39,000 experiments.

6030. Comparing that with several millions?—Of course, this is rather vague; do we really know that there are several millions?

6031. Oh, yes?—Do we know how many are castrated every year?

6032. We know to a million or two, but there are many millions. You must not think I am exaggerating about it; you will find it from the statistics returned by the Board of Agriculture every year. Assuming that one million animals are thus castrated in a year, is not the amount of animal suffering thus caused greatly in excess of what is caused by physiological and pathological observers?—The torturing is often short; you have to take all that into account.

6033. Do you really mean to say that if an animal has its testicles cut out the pain is very short?—I am not very well qualified to give an opinion.

6034. (*Colonel Lockwood.*) We will take that as your answer?—If you please. I do not think that two blacks make one white, if I may use a vulgar expression, and if you find other forms of cruelty existing it does not justify any particular one. And one can only do a few things in one's life.

6035. (*Sir John McFadyean.*) But I only wanted to be satisfied whether you would think that this particular form of inflicting pain which is known as vivisection is quite as justifiable as other instances in which pain is inflicted with public sanction. That is my justification for putting these questions to you. Do you desire to give any evidence or to offer any opinion as to whether agriculture has derived great benefit from the knowledge gained through vivisection?—No.

6036. I think you said that hundreds of thousands of dogs have been sacrificed in Europe in the endeavour to localise brain functions?—That is a guess, of course.

6037. Do you want to put it forward seriously that 200,000 dogs were experimented upon in Europe?—I did not say 200,000, of course.

6038. I beg your pardon; you said hundreds of thousands, and I took the smallest number of hundreds?—I said possibly amounting to hundreds of thousands. I put it down at a very large number. I do not know the number.

6039. (*Sir Mackenzie Chalmers.*) Your Association, I think, recommend the abolition of the present Act; that is their general line?—Yes, abolishing the practice.

6040. Would you then rest on the general law of cruelty, or would you have special prohibitions of scientific experiments?—I personally rather believe in special prohibition.

6041. You would not rest on the general law of cruelty?—I rather think that the general law of cruelty might be difficult to enforce under the difficulties of private laboratories and the difficulty of obtaining testimony.

6042. Do you think it would be justifiable to prohibit experiment on animals for scientific purposes while at the same time omitting to legislate in regard to other and probably greater forms of cruelty?—I should like to take up each form on its merits.

6043. You see no objection to beginning with a smaller form and going on to bigger forms?—I think I have given reasons for thinking that vivisection is the most crying evil, and do not you think that all things have to be done piecemeal?

6044. I only wanted to know what the view of your Association is. You referred to certain specific experiments, and we shall have Sir Lauder Brunton as a witness, who, no doubt, will have something to say about what you have mentioned concerning him. As regards Dr. Shaw, you said, I think, that he was not a licensee?—He had not a licence at that particular time. I believe that was what happened when that question came up.

6045. Was that brought to the knowledge either of the inspector or of the Irish Government, as I take it it would be?—I do not know, I expect so; at any rate, he had to make a defence, which was that his colleague who had a licence did the work.

6046. That defence would, of course, be made to the Irish Government?—I do not know.

(*Dr. Wilson.*) Yes, it was.

6047. (*Sir Mackenzie Chalmers.*) We should know nothing about it here. Then you mentioned some very horrible experiments performed in America by Dr. Watson?—Yes, where dogs were thrown down.

6048. But you know nothing more about them than the report which you saw in some paper?—It was criticised in the "British Medical Journal."

6049. Are you aware that in America there is no regulation at all as to experimentation on living animals?—I am aware that there is no Federal regulation. There are various regulations in different States, I believe.

6050. So far as I know, only in the district of Columbia?—I read the other day that there was one in Massachusetts.

6051. It must be a very recent one?—Yes, I believe it is. Broadly speaking, I agree with you that there is little or no legislation in America on the subject.

6052. In the majority of the States there is none?—That is so.

6053. And presumably these experiments take place in an unregulated place?—Yes.

6054. Such experiments, of course, could not take place in England, under the present Act?—That would depend upon whether the Home Office granted a certificate to that effect; I hope they would not; I quite think they would not. I suppose the Home Office would refuse a licence?

6055. I do not suppose that the President of the Royal College of Physicians and the authorities who have to sign applications would sign an application, and I do not think the Home Office would pass one; but that is speculation?—I quite agree.

6056. You referred to some of M. Pasteur's experiments. In France, again, you are aware that there is no regulation dealing with the subject?—That is true.

6057. And as regards Dr. Borel, all you know about him is from a letter signed in the name of Dr. Borel which appeared in a newspaper?—Yes, that is all I know. *Mr. J. W. Graham, M.A.*
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6058. You know nothing more about him or about the circumstances?—No, that is all I know.

6059. Then you referred to Sir Joseph Fayrer's experiments on snake bite. They were all, were they not, for the purpose of discovering an antidote?—Yes.

6060. We lose in India something like 21,000 people a year and an enormous number of domestic animals. Do you think experiments are justified for the purpose of decreasing that mortality and suffering?—It looks very attractive, of course. What I pointed out was that the research had been on an enormous scale, and had been going on over an interval of 120 years, and we know that there is still difference of opinion on the results, although I thought Mr. Stephen Paget made rather a good case in his book on that subject. I should attack the problem in another way.

6061. Nature is carrying on these painful experiments every moment in India, is it not?—Yes, indeed.

6062. Are we justified, do you think, in endeavouring to strive against evils inflicted by Nature?—We are part of Nature, and our duty is to strive against evils which occur in the rest of Nature.

6063. As regards this particular thing—snake bite—are we justified, do you think, in striving to find remedies?—Yes.

6064. But those remedies can only be tried on living animals, can they? For instance, I take it that there are two lines of remedy: in the first place certain drugs are tried?—Yes, this is India, not England. If we are going to limit the affair to the English Act, as I was told just now, perhaps it would be better to do so.

6065. The point arises in this way: that a good many experiments have been authorised in England with a view to helping India?—Yes, they have, but do they not take place in India?

6066. No, they take place here in England. I know that experiments have been authorised in England for the purpose of helping India. Do you think that those experiments are justified or not? For instance, there have been experiments performed with permanganate of potash; do you think that it is justifiable to try the effect of permanganate of potash on snake bite?—On a person bitten by a snake.

6067. I mean on an animal, to see whether we should be justified in trying it on a human being?—Permanganate of potash is not a dangerous thing to try on anybody.

6068. But you would have to inoculate the animal with snake poison first in order to see whether the permanganate of potash operates as a remedy?—I speak as a fool: would not the best way be to use it on a person who had been bitten?

6069. That would be to risk the life of that person if you are applying the remedy without a knowledge of its effect, instead of applying other remedies?—I am aware that there must be a certain amount of experimentation throughout medicine, obviously, in trying things, and it is always a delicate and careful question with a medical man, no doubt, as to how far he ought to go. I quite admit that sort of difficulty which besets the medical profession; one cannot but sympathise with it.

6070. I want to know whether you would go a little further than that. Supposing there is a new drug which is thought likely to be a useful drug in medicine, do you think it is justifiable to try it on an animal before you try it on a human being; or do you think that it ought to be tried on a human being first?—It all depends, I think, upon the case.

6071. Take the case of a new soporific?—I should myself have no objection to trying it on an animal first. I do not know that all my friends would agree with me in that. I feel just a little hampered in my answers by the fact that I am not answering entirely for myself. I think I would answer some of your questions a little more freely if that were the case.

6072. I rather wanted your own individual opinion?—I think that I would not at all object to trying a soporific on animals.

6073. Then where are we to draw the line?—I rather think that I have already admitted what a difficult problem that is, and I suggested that the law should

Mr. J. W. Graham, M.A. leave that to the decision of the Courts; that the business of drawing the line should be, and naturally would be, left to the judiciary.

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6075. What direction would you give to them?—They would have to interpret a carefully worded Act.

6076. But what would be the lines on which they would interpret that Act?—I know quite well that this is the weak point of my case; but in dealing with a large question like this you ought, I mean to say, to deal so far as you can with the large issues and not with the borderland cases. I will go so far as to say that if nothing had ever been done in the way of scientific experimentation worse than the ordinary trying of drugs there never would have been an anti-vivisection movement; it would not have been worth powder and shot. The movement arose out of far more serious matters than that.

6077. Take the case of an operation. I understand that if an operation is performed under anæsthetics and if there is no serious pain afterwards, assuming that the animal is allowed to recover, you see no ethical objection to it?—I do not.

6078. Is not that condition now realised in the vast majority of cases?—That is such a loose expression that one hardly likes to reply to it.

6079. (*Dr. Gaskell.*) I did not quite understand, in regard to the experiments of Sir Lauder Brunton and Dr. Theodore Cash, whether you mean that the cruelty was because no anæsthetics were given, or because the anæsthetics were inefficient?—There is no mention of anæsthetics in the article.

6080. But do you imagine yourself that no anæsthetics were given?—From the nature of the experiments I think there hardly can have been any anæsthetics given.

6081. Then we will ask Sir Lauder Brunton about that when he comes. You also said that you could have given a great many more cases of cruelty if you had looked them up?—Yes.

6082. Would those cases which you would have given be cases of cruelty because no anæsthetics were given?—You are asking me about cases which I have not brought forward?

6083. Yes, because you say that you could have given many more. Would you have included, among those, cases in which anæsthetics have been given?—It would all depend upon the permanence of the anæsthetic effect.

6084. But I rather understood you to say that the reason why there was cruelty in many experiments conducted in physiological laboratories is, either because morphia was given, or because you did not trust the anæsthetic; was that not so?—I do not always trust the anæsthetic.

6085. I may take it for granted, then, that a good many of the cases of cruelty, such as Mrs. Cook gave us the other day and such as you have given us to-day, were cases of cruelty because the anæsthetic was not in your opinion sufficient?—My impression is that that occurs. My cases you observe were not of that character.

6086. Yes, I beg your pardon, they were. In your answer to me about Leonard Hill's experiments you said that only morphia was given?—That is not using an anæsthetic; that is using a narcotic instead of an anæsthetic.

6087. I beg your pardon, morphia is an anæsthetic?—There is a difference of opinion about that, is there not?

6088. Very well. I understand that you agree with other witnesses who have come here that there is a very great difficulty in anæsthetising animals—that there is a difficulty in keeping animals completely under anæsthesia?—Yes.

6089. Do you consider that in hospitals men cannot be kept under anæsthesia?—Oh, yes, men can be kept under.

6090. There is that difference then, you think, between men and animals?—I should imagine that there is a difference also in the amount of care that is likely to be taken in the case of men.

6091. Why do you know that about men? How is it that you are able to speak confidently in the one case

and not in the other?—Because we know that those who are operated upon in hospitals do not as a rule suffer pain.

6092. How do you know that?—Because we have never had complaints that they do.

6093. Is it not because of the statements of those in the hospitals? Is it not because of what you know of the work done in hospitals?—Never having heard any complaint, one would assume that there is no cause for it.

6094. Then the next question I want to put to you is this: There are hospitals also for animals—veterinary hospitals, and there are also their operators. Do you imagine that those operators operate on animals without the animals being unconscious?—I make no statement on that point.

6095. But I want to know. You say that you have reason to believe that in the case of human beings you can get anæsthesia in hospitals?—Yes.

6096. Have you not similar reason to believe that in the case of animals you can get anæsthesia in their hospitals. Have you any reason whatever to suppose that the veterinary operations are cruel?—We know that the danger to life in the animal is such that very often an anæsthetic is not used.

6097. I am asking you a simple question. You are content to believe that in the case of human hospitals there is no cruelty because the anæsthesia is complete?—Yes.

6098. I put it to you: Does not the same evidence content you in the case of the veterinary hospitals?—Of course, we have not got the same evidence, because the dogs cannot talk as the human beings can.

6099. But have you satisfied yourself in any way on the subject; have you any doubt as to veterinary surgeons being able to operate in the veterinary hospitals without causing pain?—I have read the testimony of veterinary surgeons here before this Commission who have been examined, and who said various things on that point, which I accepted, and from which I have learnt more than I knew before.

6100. In other words, you tried to learn from that evidence?—Yes.

6101. Then why did you not try to learn before you came here whether your suggestion that the physiologists did not insure complete anæsthesia, and that the animals were therefore in pain because the anæsthesia was so difficult to get in animals was correct or not?—I did. It was not since I came here that I read the published proceedings of the Commission last autumn.

6102. But you have apparently tried to find it out with respect to simply one man—Dr. Borel. Why did you not go to the veterinary people and ask them definitely, and so find out that anæsthesia is perfectly possible in animals just as much as it is in man?—I think that no one says it is impossible. A great many people think that it is uncertain and that sufficient attention may not be paid to it.

6103. Would you recognise that anæsthesia is carefully given in all physiological laboratories, according to the very statements of the observers themselves?—I do not know whether you will accept this evidence here—probably you will not, and I did not bring it forward—but I have here an extract, which if you wish me to read I will read, from *L'Encyclopédie Contemporaine*, consisting of an interview with Professor Chantemesse. I would really rather not read it, but it is in answer to your question.

6104. It is not an answer from an English source?—No.

6105. (*Dr. Gaskell.*) I think you said that you had been studying this question for from 15 to 20 years?—Yes.

6106. Then am I to understand that you have not attempted to find out from veterinary surgeons or others as to whether it is possible or not to completely anæsthetise animals?—I have read a great deal of testimony on both sides.

6107. You have not attempted personally to find out; you have not gone to anyone or seen or asked him; the only evidence you have given us is that of Dr. Borel?—That is his evidence.

6108. That is evidence which is of very little value?—That is as you think, of course.

6109. I should like to know whether you have ever attempted to find out—there are plenty of people round about you in Manchester—as to whether the statement that anæsthesia is so difficult to secure in animals is true or not?—I should not think that an answer which I might get from a neighbouring veterinary surgeon would be of so much value as the answer that you get from the published statements of more important people.

6110. Still a veterinary surgeon is a person, of course, who is in the habit of doing this?—So are the people who have made the published statements to which I refer.

6111. You advocated, so far as I could gather, that these operations of castration and spaying should be done under anæsthetics?—Yes.

6112. You would not have advocated that, of course, unless you supposed that the anæsthesia was complete?—The operation is so short that the anæsthesia might more easily be complete.

6113. Then the difficulty in your mind is, keeping the anæsthesia for a length of time?—I have always understood that the difficulty is in keeping the animal for a long time in a state of anæsthesia without killing it.

6114. But you have not looked into that matter and attempted to find out?—Excuse me. I have not called upon any neighbouring veterinary surgeon.

6115. You have not asked anybody who is acquainted with the subject?—I should not admit that. I cannot recall everyone to whom I have spoken for 15 years on the point.

6116. Still you have given us one authority, that is only Dr. Borel. I should have thought that if you had got definite authorities you could have given us some more?—If the main purport of my evidence had been on that subject I should have given you more.

6117. Then there is another thing that I should like to know. You spoke of these experiments in physiological and pathological laboratories as influencing the morality of those who practice them. Have you ever been to any physiological laboratories or pathological laboratories, and have you ever seen any of those experiments?—I have been to laboratories, but I have not seen experiments going on while I was there. We have laboratories at Manchester, of course, but I do not know that I should be admitted to such a laboratory whilst work was going on.

6118. I do not think that there would necessarily be any difficulty?—Besides, of course, the private research laboratory of the professor is where one would have to go, and I have not intruded into such a laboratory.

6119. Are you personally acquainted with vivisectioners?—Yes.

6120. Do you find them an immoral set of men?—I think their work has a bad effect upon their characters in that particular direction.

6121. Are they not fond of animals?—No, I should have thought not—not in any true sense.

6122. Do not they generally have dogs of their own?—I have not inquired.

6123. I think if you do inquire you will find that they are exceedingly fond of animals, and that they are as kind-hearted to animals as any other people?—One rather goes by their actions.

6124. Have you ever come across any physiologists who have abstained, for instance, from shooting animals or hunting foxes because they object to the cruelty involved?—It takes some time to grasp what you are driving at.

6125. I mean to say that I have known physiologists who, in consequence of their feelings towards animals, have abstained from field sports because of the cruelty involved in those field sports?—I have never known of such a case, though I quite admit that it may be so.

6126. That would be a high moral sense, in your view?—Yes, it would; but human nature is so complicated that you might find almost any statement of that kind true with regard to any body of men, I think.

6127. I have yet to learn myself, and I was wondering whether you could say, that physiologists and experimenters of that kind are less humane than other people?—Well, it is my opinion that they are.

6128. But you have not much experience of that class of men?—I have some; but if I were to go into details it would become rather personal.

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6129. It would, very. But that is your opinion?—Yes.

6130. I think you are unique in it.

6131. (Mr. Tomkinson.) You began by telling us of some experiments which were stated to have been done by Dr. Lauder Brunton on rabbits and cats. You called them baking experiments. Can you describe to us what the nature of them really was? I understand that these animals were put into a very hot atmosphere?—A kind of stove.

6132. In order to investigate the effect upon their blood temperature?—Upon their pulse.

6133. Then they would be taken out, I suppose, and tested?—I suppose so. I do not know.

6134. But you do not wish us to understand that they were slowly baked to death or roasted inside?—They were heated until they died; that is the form of words used.

6135. Until they died?—Yes; I think Sir Lauder Brunton will tell you about it as well as I can.

6136. I wanted to hear your account of it?—I confine myself to the narrative in the published article.

6137. Do you know what the normal temperature of a rabbit is?—No.

6138. Nor do I, but I know that the normal temperature varies in different animals; that of a horse is 100°—that is 2° above human beings—and that of the rabbit, which is a very warm thing, might be much nearer 113°?—There is a chart given, and I gave you the highest.

6139. But the mere fact of a temperature of 113° would not be cruelty or torture in a heated place. You are aware, I suppose, that people go into a Turkish bath and sit in a temperature up to 200° and 220°. I have sat in 220° myself. In the first room the temperature is 125°, in the second one 150°, in the third 170°, and in the fourth up to 200°, if it is dried heat?—I was speaking of the temperature of the blood of the animal.

6140. You do not wish us to understand that these poor creatures were slowly baked to death, because that could not be under anæsthesia, and it would be most awful torture?—They were slowly done to death by heat.

6141. What was your case of the irritation of an eye?—That was from a paper by Dr. Cecil Shaw, which is published on page 158 in the "British Medical Journal" of the 18th June, 1898, giving an account of how "for varying periods up to six months"—those are his words, although I noticed in another part of his article he says that he killed a good many after three or four months; I cannot quite reconcile the two statements, but these are his exact words—"for varying periods up to six months he rubbed an irritating drug, jequirity seeds, causing (I did not mention this) purulent conjunctivitis, into a puncture he had made in a rabbit's eye with no antiseptic precautions." That was because he wished to reproduce the conditions of the actual wound; that was for external irritation; and then afterwards for internal irritation he pushed small shot into the rabbit's eye, carefully using soiled instruments. It was a study of sympathetic ophthalmia, and the words I have from him are "producing prolonged irritation of one eye."

6142. (Colonel Lockwood.) Was that done under a licence?—That was the one for which he had not got a licence.

6143. (Mr. Tomkinson.) Then there was some discussion on the expression "period of observation." We have heard before this Commission a great deal of prolonged periods of observation in very minor operations, for simple inoculations, in fact in many cases in which the inoculation had no effect at all—simply the animal was not hurt, and was kept under observation to see what the effects of inoculation might be. But the case to which you referred was a very serious operation?—Yes, it was.

6144. And it was a case in which you said that either whole or partial anæsthesia was kept up during it, or ought to have been kept up. Was that after a serious operation?—Yes, the throat was cut and cannulæ inserted, and a large part of the viscera exposed.

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6 Mar. 1907. 6145. What was the nature of the operation?—It was a question of the effect of gravity on the circulation; but they exposed a good deal of the body of the animal.

6146. In the centre of the animal—the heart?—Yes, in the centre of the animal. They used chloroform whilst it was being done, and then they had to observe the pulse for a good while afterwards.

6147. Was the heart exposed?—I do not know.

6148. (*Sir John McFadyean.*) Morphia was used?—Yes.

6149. (*Mr. Tomkinson.*) And the animal was kept more or less narcotised?—The account says “morphia was used.”

6150. During the period of observation?—Yes.

6151. It does not say how long the period was?—No; at least I have not got it here.

6152. It would be a question of hours, but not days?—I should think so.

6153. You said that it was no part of your business to inquire as to the use or uselessness of the system of vivisection?—No, I have avoided that.

6154. Or the adequacy, or supposed adequacy, of the results?—I have left that to other witnesses.

6155. You said, in reply to the last questioner, that you had never yourself witnessed any of these operations?—No, I have not.

6156. Do you think it would be in the interests of the public and for the quieting of the public mind if permission were given to properly authorised laymen, members of Parliament or others, to see these operations and to satisfy themselves as to the efficacy of the insensibility or otherwise?—I think so; I think it would be very useful.

6157. You said that your Society would not approve a Bill simply excepting dogs from the subjects of experiment?—They would not value it particularly; that

is not their policy. Of course, one would value anything in a certain sense.

6158. I quite understand. It is not that you object to an instalment?—I personally do not object to an instalment, but this Society which has asked me to come here has no policy except the complete policy.

6159. I quite understand—know all about it; because it would be obvious to you, would it not, that even if the Society could attain the summit of its ambition in the total abolition of vivisection by law in the United Kingdom, still it would leave the whole of the rest of the world open to the practice?—It would.

6160. Therefore it would only be getting a very small instalment for the animal creation generally?—Quite so—relatively small.

6161. But it would not be wise to reject a half loaf in the shape of the exception of dogs, if it could be obtained, although I quite understand that it is not the aim of your Society?—That is so.

6162. You were asked whether you object to the use of drugs or sera or lymph for the prevention or cure of a disease now, on the ground that such a drug or remedy has been discovered as the result of experiments on living animals, with the consequence of pain to them?—I think I answered, after a good deal of hesitation, that, to speak strictly, one has no moral right to use them; but I feel that it is rather a hard question.

6163. Then may I put it in this way—that the opposition is on this ground: that while you feel that it is impossible to go back upon what has been done, you do object generally to the animal creation being exploited painfully for the discovery of either prophylactics or cures of diseases in human beings?—Yes, when it is attended with great pain.

(*Dr. Wilson.*) I think, Mr. Graham, you have given your evidence in such an able, straightforward, and candid fashion that I have no questions to put to you.

FIFTEENTH DAY.

Tuesday, 12th March 1907.

PRESENT:

Col. The Right Hon. A. M. LOCKWOOD, C.V.O., M.P. (*in the Chair*).

Sir W. S. CHURCH, Bart., K.C.B., M.D.

Sir W. J. COLLINS, M.P., M.D., F.R.C.S.

Sir J. McFADYEAN, M.B.

Sir M. D. CHALMERS, K.C.B., C.S.I.

Mr. A. J. RAM, K.C.

Mr. W. H. GASKELL, M.D., F.R.S.

Mr. G. WILSON, LL.D., M.D.

Captain C. BIGHAM, C.M.G. (*Secretary*).

Miss ARABELLA KENEALY, L.R.C.P., recalled; and further Examined.

Miss A. Kenealy, L.R.C.P.
12 Mar. 1907. 6164. (*Colonel Lockwood.*) There is only one thing I want to ask you. You will keep as close as you can to these numbered paragraphs, because the evidence is already voluminous. Does this represent your individual ideas, or the conglomerate opinion of every society that you represent?—My own ideas.

6165. Simply your own ideas?—Certainly. Medical science and the evolution of the race have been the passion of my life. May I say that I was right in supposing that any number of animals may be, and are, used to demonstrate or to refute a single theory, but that it is customary to describe each animal as an experiment. Hence the confusion. For example, in Swale Vincent's and W. A. Jolly's experiments on the functions of the thyroid and parathyroid glands 27 animals were used, six monkeys, six cats, nine dogs, two prairie wolves, two badgers, and two rats; that is in the experiments I was speaking of on the functions of the thyroid and parathyroid glands.

All these animals were sacrificed in order to show one thing, namely, the influence of the thyroid and of the parathyroid glands, and to prove that the numerous previous experiments had resulted in mistaken views as to the importance of these glands. Sir Victor Horsley is quoted in “The Hospital” for November 26th, 1904, as having said that the only satisfactory result of all these innumerable experiments upon the thyroid and parathyroid glands has been the treatment of myxedema by means of thyroid extract. But surely all these mutilations of miserable creatures were an absurdly roundabout and impracticable way of showing that in disease of a particular organ benefit may be derived from administering to the patient an extract of that organ. It has been done for years with pepsin. Then I find that I was right too in stating that in the descriptions of experiments the number of animals used may be, or may not be, given. In those ridiculous injections of shark and squeteague liver and of star fish into dogs, which I described in my previous

evidence, the number of dogs is not stated. Nor is it stated in the suprarenal gland experiments. Four rats are specified as having been used, six rabbits, four cats, and four kittens—

6166. (*Dr. Gaskell.*) If I may interrupt you for one moment—those experiments you speak of, the squeague experiments, were done in Canada, were they not?—Yes, they were. And only a few “examples” of the guinea-pigs are given, although it is clear from the paper that a very large number of guinea-pigs were mutilated. Then I was not able to give you the percentages of the decline of interest in treatment, which I spoke about. But in a presidential address to the Clinical Society of London, Dr. F. Taylor classified the contributions to it for the preceding 10 years. Of the surgical papers—which outnumbered the medical—(this is significant, a surgical operation frequently meaning a failure in medical skill to arrest disease in its earlier stages) three-fourths of the surgical papers, he said, dealt mainly with treatment, while of the medical papers only one-fourth made treatment their main consideration. “The Hospital” of October 29th, 1904, in commenting upon this says:—“It will be admitted that this is a somewhat unfortunate state of affairs, as it cannot be considered as other than indicative of a more or less general professional tendency. There are aspects of medical work which to many are much more attractive than the question of treatment, yet the latter necessarily remains as the main responsibility of the practitioner. Even in the portion of the curriculum specially devoted to it there is nowadays a disposition to cultivate a strictly scientific and often barren pharmacology at the expense of practical therapeutics.” Then may I read the Report of the Lister Institute, in which it speaks of antitoxin causing paralysis, “The increased number of cases of paralysis following the use of antitoxin has by certain individuals been attributed to the use of antitoxin.” But are not these individuals justified in attributing to the use of antitoxin this increased number of cases of paralysis which have followed the use of antitoxin? How else are we to account for the increased number of cases of paralysis which have followed its use?

6167. (*Sir Mackenzie Chalmers.*) What is the date of that Report?—1904. The Report goes on “Experimentally it has been shown that the antitoxin, if given early enough, protects against the paralyzing substance.” But can we consider experimental findings upon guinea-pigs worth a rush in the face of the fact that in clinical practice the use of antitoxin has been followed by an increased number of cases of paralysis? And Bosanquet has said, “Antitoxin does not, however, neutralise the toxic material causing paralysis.” Again, the Report goes on “As experimentally antitoxin protects against the paralyzing substance, the increase in the number of such cases must be attributed to paralysis occurring in cases which, had antitoxin not been used, would have proved fatal in the early stages of the disease.” Now, this is very ingenious, but I think we may wholly reject it, seeing that there is nothing whatsoever to support it beyond certain experiments on animals, which, as has again and again been shown, are quite untrustworthy when applied to human diseases.

6168. (*Sir William Church.*) Still, I suppose you would not deny that the severe cases of diphtheria are those which are generally followed by paralysis?—Not always, I think. I think in a great many quite mild cases paralysis follows.

6169. But that is your position. You say that the toxin of diphtheria does not produce paralysis of itself?—No, I do not say that. Certainly not.

6170. Then how do you explain the larger number of people that recover from diphtheria? You say that paralysis is caused by the antitoxin. How do you show that it is not caused by the disease? Many of these cases which it is supposed, perhaps erroneously, would have died without the use of it?—My position is that in all diseases nature selects the organ that shall throw out the poison. In typhoid fever it is the glands of the small intestine; in scarlet fever it is the skin and throat; and so forth. In diphtheria nature has selected the tonsils as the best route for eliminating the poison. By giving antitoxin you may relieve the tonsils, but you throw the poison back into the blood, so that it causes paralysis; it causes albuminuria, it causes convulsions and sudden death, rashes and inflammations of joints. I think, therefore, that it is an unscientific method of treatment.

6171. I think I understood you when you were good enough to give evidence before to say that you did not believe that any serum treatment was of use in disease?—Yes, distinctly.

6172. That is your opinion?—That is my opinion.

6173. Would you make the use of the serum treatment illegal?—I would make it a legal offence.

6174. That indicates your position very clearly?—I think it is so very dangerous.

6175. How have you arrived at that conclusion?—Take the case of antitoxin alone.

6176. You do not seem to me to have had any practical experience whatever of this treatment, have you?—Of the antitoxin treatment?

6177. Of any form of serum treatment?—I have followed the results of it in the *British Medical Journal* and other medical journals.

6178. That is to say, you have had no practical experience?—No practical experience.

6179. You have not been in contact with any of these cases yourself?—No, I would not countenance such a treatment.

6180. Can you give us the evidence of anyone who has had experience of the treatment of diphtheria, both before and after the introduction of the serum treatment, who supports your view?—I require no evidence, I think, except the Report of the Lister Institute itself upon the subject of the action of antitoxin if it increases the number of cases of paralysis.

6181. But if you would kindly answer my question; I asked you if you can state before the Commission here that you can produce the evidence of anyone who has had experience in the treatment of diphtheria, both before and after the introduction of the serum treatment, that it is useless?—May I read you something?

6182. No. I wish to know whether you know of anybody who has been in that position and is prepared to come here and tell us that it is useless?—I know very few doctors who will do anything in that way.

6183. I want to know of someone who has had experience of the treatment of diphtheria?—They take the conventional view; they do not take an original view.

6184. (*Colonel Lockwood.*) Have you, or have you not, known such medical men—you have not, probably?—I know few such cases. I have been out of practice for some years, but I think the onlooker sees more sanely.

6185. You prefer the views of the onlooker; you think he is more likely to know?—No, I do not. But as an onlooker, who looks at the results of practical men, and from my own reasoning, I deduce that these serum treatments are most dangerous.

6186. (*Sir William Church.*) I will not trouble you any further on that point. Would you extend your objection to the use of sera on animals?—Do you mean for treatment?

6187. In the treatment of animals, yes?—Certainly.

6188. Have you any experience of the treatment of animals by sera as a preventive of disease?—No, but I see in medical science this absurd anomaly—

6189. But you have no experience?—But will you, please, let me say this—

6190. That is not the answer that I want—you have no experience?—Will you let me say this, please?

6191. (*Colonel Lockwood.*) Yes?—On the one hand we have our surgeons soaking their hands in hot water for ten or fifteen minutes, and in strong antiseptic solutions in order to prevent themselves from conveying morbid products from one human being to another—

6192. (*Sir William Church.*) I am not speaking of human beings at all?—On the other hand we have our physicians injecting directly into the blood of human beings the morbid products of the lower animals, rendered artificially diseased.

6193. But I was not asking you anything about that at the present moment. I asked you whether you had had any experience of the use of sera as either curative or protective means of treatment in the case of animals?—No, of course I have had no experience at all of the treatment of animals.

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6194. Then do you think that your opinion, which is gained from apparently very wide reading, is to be placed against the opinion of those who are in daily observation of these diseases in animals?—If you look through the records of cures you will find that they are most delusive. The Christian Scientists have marvellous cures; the Homœopaths have marvellous cures. One cannot depend upon these records.

6195. I do not see what that has to do with my question, but never mind. You spoke of centuries of experiments upon the brain and ductless glands producing no results. What did you mean by that? What are the experiments that have been carried on for centuries on the brain and ductless glands?—There is no doubt that experiments have been carried on, upon the ductless glands, for centuries.

6196. By whom, or when?—Upon all the organs of the body. Vivisection is a survival of barbaric times. It was the first method adopted; it is, if I may say so, the method of savages.

6197. Are you aware of what the earliest experiments with which we are acquainted, which throw light upon the functions of the brain, are?—It was Alkmaon who first placed the seat of consciousness in the brain, and he did not do that by experiments on animals. 580 years B.C. he placed the seat of consciousness in the brain, not as the result of experiments on animals, but by that intuitive knowledge which Nature gives, which leads us to truth.

6198. Are you acquainted with Galen's experiments upon the brain and nervous system?—I am quite prepared to admit that in those days, when dissections were not carefully made, and before we had the microscope, vivisection may have contributed something to medical science.

6199. But still, what was known of the functions of the brain was really obtained by experiment. Did not Galen divide the spinal cord, and then it was found that the power of voluntary movement was abrogated?—As I say, it may have been so in those days in just the same way as in those days it was customary to maintain law and order by mutilation. But mutilation is a survival from barbaric days.

6200. I do not want to get into an academic discussion?—I think the cases are analogous.

6201. I merely want an answer to my questions. As a matter of fact, is it not the case that the first knowledge we had of the functions of the brain was derived from direct experiments?—Kirkes says that Alkmaon discovered the seat of consciousness, and it was not by experiments on animals. I think that was the first knowledge of value.

6202. Would it have been possible for what we now term physiology to be in advance of the other sciences?—I do not think that physiology is in advance of other sciences.

6203. But would it have been possible?—I think it would.

6204. You state as one of your objections to vivisection that experiments for centuries have led to no results, and I ask whether it would be possible for physiology to have given results in advance of chemical and physical knowledge?—Well, after all, the greater part of the physiology we have is the physiology of cats and dogs and rabbits. And I do not think these creatures are of sufficient interest to us.

6205. I will put my question rather in this way: Would it have been possible for physiology or, if you like to call it so, medicine, to have made any very great advances before we were acquainted with circulation and the nature of respiration?—No, I think not; but then I think those truths were to be found by other methods—by observation of human beings, by making post-mortem examinations, and by pathological findings.

6206. Therefore your paragraph about centuries of experiments does not mean anything?—I thought it did.

6207. I suppose it would have been impossible, would it not, for us to have arrived at anything like our present knowledge of what respiration effects, before we were acquainted with the properties of oxygen?—I think so; that certainly is true; but I think it is most important that we should know more about the physiology of human beings. Perhaps three or four millions of years have elapsed between the physiology of cats and rabbits and the physiology of

human beings. We want our physiologists to show us the developments which must have taken place in those millions of years, perhaps three million or even ten million years; and our physiologists simply keep telling us the physiology of rats, and cats, and rabbits. That, I think, is the great mistake that physiology makes.

6208. (Colonel Lockwood.) When you are talking of millions of years, you are referring to evolution?—Yes.

6209. (Sir William Church.) We will leave that. Have you any objection to feeding experiments?—Certainly not if they involve no cruelty. I object decidedly to feeding beautiful little healthy Jersey calves with the sputa and glands of tuberculous diseased persons. I think that the moral sense of the man who could do that must first have been blunted.

6210. Have you ever yourself watched a case of myxedema, or the cretinoid condition of children being treated by feeding with thyroid extract?—I believe that it is extremely beneficial.

6211. Have you ever yourself watched a case?—No, I have never seen a case fed, but I am quite willing to admit it. But vivisection was not necessary to prove that the administration of a gland may be valuable in disease of that gland.

6212. I was asking you about feeding experiments, not of vivisection. Still, before we can form any opinion as to the way in which a substance acts, it is necessary, is it not, to try and experiment in some way with it?—Yes, but I think those experiments should be upon human beings—upon ourselves.

6213. But we could not deprive a human being of his thyroid gland and then feed him?—No, I was not meaning that; but the depriving these creatures of their glands has had such various results in various creatures, and in the hands of various operators, that it is no indication at all to us of what the function of the thyroid gland is.

6214. You deny that any increase of knowledge has been arrived at with regard to myxedema and thyroid feeding by experiments on animals?—I do not say thyroid feeding. But in these operations on the thyroid and parathyroid glands, all the observers get different results. Some produce tetanus and myxedema, others do not.

6215. Is it not the case always in any discovery that the observers get different results at first until the thing is worked out?—I think in the case of vivisection it is a question of the man who maintains his opinion the longest; not that one man proves it, but that another man eventually gives up the contention as worthless, and then the opinion of the first man prevails.

6216. Can you point to any discovery in science at all that has been made at once by a single experiment and not controverted?—No; but I think we have only to look round upon all the degeneration and disease, and our absolute helplessness to do anything for it, to see that medical science must have been adopting wrong and misleading methods.

6217. The doctrine is wrong, and therefore everything that comes out of it must be wrong also?—I think that is true. Evil begets evil.

6218. Have you ever yourself used adrenalin, or seen it used?—No, but I should find it quite unnecessary. We have quite enough beautiful healing medical substances from the mineral and vegetable worlds without going to the bodies of the lower creatures. I think there is a great deal of danger in it. If you will let me read you an extract, I can show you the danger as it is pointed out by an expert.

6219. Who is the expert?—The experts are Professor Von Poehl, Professor Prince Tarchanoff, and Dr. P. Wachs, who say that there is very great danger in administering these morbid products of diseased animals.

6220. I will accept that without our going into it; but you yourself have never used adrenalin, and you would class it with other animal products. Would you carry that so far as to include cod-liver oil?—That is a food. I think that is quite a different thing. The oil is a food which is not given subcutaneously; it is given *via* the stomach, and the stomach is a very intelligent organ and discriminates between things which are good and things which are bad to be passed into the system.

6221. Therefore you would have no objection, and you would think it would be right, to give extracts of suprarenal capsules by the mouth?—There is not the same objection to that that there is to injecting subcutaneously. But I do not think it is necessary. The vegetable kingdom has given us opium, the vegetable kingdom has given us strychnin, the vegetable kingdom has given us belladonna—all these and hosts of other beautiful healing medicaments. It is quite unnecessary to go to diseased lower creatures for our medicines.

6222. You have never seen the application of adrenalin to a mucous surface, I suppose?—No.

6223. Did you, when you were in practice, place any value at all upon an alteration of the heart sounds?—Yes, I hope I practised with intelligence.

6224. Do you know how our knowledge of them was demonstrated and proved?—I know that after Harvey (Harvey, I believe, was 300 years ago) we believed for 300 years in certain movements of the heart which altered the position of the apex beat, till Haycraft discovered that those movements only occurred when the chest wall had been removed, and that they did not occur in the normal heart. That was most misleading.

6225. Are you aware of the way in which the value of the different sounds in the heart (what we call medically, murmurs) were demonstrated? They were recognised for a long time, but their true interpretation was not demonstrated and shown and proved until animal experimentation, was it?—I think that showed a singular lack of intelligence.

6226. I am asking you whether you think they were or were not discovered?—If you ask me what I think, I think all these things were discovered by pathology; by listening to the heart in disease and in health, and comparing the two, and that that is the only valuable method.

6227. Then you think the experiments of Dr. Hope and Dr. Williams had no effect in demonstrating the truth of various theories, or the correctness of various theories which persons who had only listened had formed?—I think if they had they were unnecessary; that by listening to the heart in health and in disease we could get all the indications we needed.

6228. Do you carry your objection to these products from animals to such an extent that you disapprove either of experiments with or the use of the streptococcal and staphylococcal fluids?—Certainly.

6229. Even when they are obtained from the patient on whom you use them?—Yes.

6230. On what grounds?—If they were of any use to the patient, the patient himself is manufacturing them and he would be able to manufacture them for his own use.

6231. It would add to his manufacture, perhaps?—I doubt it.

6232. You have had no experience; you have never seen the certainly somewhat startling results that sometimes seem to follow—I only say, seem to follow; we cannot say that they certainly do?—I think that when hypodermic injections are given it is quite impossible to calculate the result; that there is no doubt of the hypnotic influence of the touch of the doctor; and although it is not generally recognised, I believe that there are healing forces in certain people, and that very often these produce effects which we attribute to drugs which have been given hypodermically.

6233. You have never seen any results yourself from the use of these products?—I have seen a patient who expressed himself very much benefited by having a clinical thermometer in his mouth for a few minutes.

6234. Oh, I am not talking of such a thing as that?—It shows the effect of the mind upon the body.

6235. You think it is wrong to use them. Would you go as far as you do with the sera? You see there is no danger in a case where you obtain the material from the patient himself of introducing any animal susceptibility?—I object to the whole thing. I think it is an unwholesome and morbid method of treatment.

6236. Do you disapprove of experimentation for diagnostic purposes?—I disapprove altogether of using sentient living creatures as subject for experiments.

6237. That is to say, supposing you have a case, as happens to practitioners, in which you are in doubt.

I will give you a case—a case in which you have got conjunctivitis and you are uncertain whether it is tubercular or not, do you think it is wrong to experiment on an animal to see whether that discharge from the conjunctiva is tubercular or not?—Certainly I do. I think it is an abominable act.

6238. Then you would prefer that your patient should run the risks of uncertainty to experimenting upon an animal?—No, I think I could judge by the clinical aspects of the case whether it was tubercular or not. If I could not, it would be due to my faulty perceptions.

6239. But supposing you could not—supposing you were in doubt—a great many of us are in doubt—we are not sufficiently good?—Supposing you clear up the problem, what can you do?

6240. You can recommend a course of life and treatment to your patient which will give him a much better chance?—That would be very much the same in both cases.

6241. I will not press the question further; you would not do it, and you do not think it is right. Now, for the purpose of guarding the public health, do you think it is justifiable to use animal experimentations to keep plague out of the country?—I think there are other methods.

6242. As good methods?—I should say decidedly so; I believe the tuberculin test is being rejected now.

6243. I am not speaking at this moment of tuberculin. I am speaking with regard to plague?—What is true of one is true, I think, of all.

6244. Then you do not think they should be used in any case for guarding the public health?—No. I think we have other very much better methods.

6245. You say that you think so; but have you ever studied the question of public health with regard to these diseases, such as plague?—Yes, I have.

6246. I have asked about tubercle and plague. The same, I suppose, applies to cholera or rabies, or many other diseases I could mention?—I think in all these cases the clinical aspect is much more valuable, and really the only reliable one.

6247. That is your opinion?—It is my opinion; I can only give you my opinion.

6248. But it is your opinion formed without having had any experience?—It is my opinion formed from the reports of those who have had experience.

6249. You have used the word pathological in your *précis* in a way which I not quite understand. What do you mean? Do you confine the term pathology to morbid anatomy?—Not entirely. Disease in any form is pathological.

6250. Living disease?—Yes, or dead disease. It is all pathology.

6251. I rather gathered that you used the word as synonymous with morbid anatomy?—I did not mean it so.

6252. You speak of the value of the study of pathology; but surely a very great part of pathology at the present moment is the study of living organisms?—Do you mean micro-organisms—bacteria?

6253. They are some of the living organisms that you have to study—bacteria are, certainly?—But do you mean that?

6254. I mean that partly. We have other bodies we have to study besides bacteria?—Certainly.

6255. You speak, for instance, of the provision made for getting rid of malaria by draining, and so on. We are not dealing with bacteria there, are we?—We are supposed to be. There is supposed to be a micro-organism of malaria.

6256. It is not a bacterium, is it?—An organism.

6257. Do you know anything about sleeping sickness? Is a bacterium supposed to be the cause of that disease?—Not a bacterium; it is an organism of some kind.

6258. Then you use the term bacterium as equivalent to micro-organism?—Yes.

6259. I understand you. But in pathology we have to study now, have we not, the condition of the blood during life as well as after death?—Yes, of the blood of human beings, but not of the blood of guinea-pigs.

6260. Yes, I mean the blood of human beings?—But we are devoting nearly all our time now to the study

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of the effects which take place when we inject these products from the lower animals into other animals.

6261. You are not aware of any researches made recently upon man, then, if you think the medical profession is entirely confining its energies to these animals?—I am, of course, aware of the experiments and the recent results of medical science.

6262. You have frequently stated that the whole of the medical mind is now diverted to the study of these sera. You are aware that there are other things which have been studied as well?—There are a few observers of other phenomena, but I am afraid they are but few.

6263. As I put to you the other day, you are not of opinion that one great object of medicine now is to follow out the course of the disease and try and find out something of its cause and character as well as of its treatment?—I do not say that medical science is absolutely unintelligent. But I think it might be very much more intelligent than it is if it gave up all these observations on guinea-pigs and rabbits.

6264. I only want to get your opinion on one other question, and that is this: Do you think that whooping-cough and measles and scarlet fever and such like only attack weakly children and kill them off?—No, I do not. I do not say so in my *précis*.

6265. You say "For example, the child having been already tested, by the severe ordeal of dentition, is further tested for his fitness for survival by the zymotic diseases; the nervous system and respiratory organs by whooping-cough; the skin, throat (pharyngeal) respiratory organs and mucous membranes by measles; the skin, throat (laryngeal) serous membranes, and kidneys by scarlet fever, and so forth." I really do not quite know what you mean, unless you mean that the weakly ones are killed off?—What I mean to say is that those diseases occur so constantly in children—that they are almost as inevitable as dentition, and for that reason I think they may probably have some developmental effect.

6266. Do they ever leave on apparently healthy children bad results?—Certainly; but then such children evidently were not healthy. And Nature found out the weak spots and showed them to us, so that later we could take them into account.

6267. Are we right or wrong in trying to guard our children against these diseases?—By isolation, certainly.

6268. But you do not give nature a fair chance, then, to show these weak spots in children?—Nature will find them out at the best opportunity.

6269. In fact, you think it is for the advantage of children that they should have these diseases?—I am not sure. But as they always occur, I cannot help thinking they may have some developmental effect, or that they are merely tests to roughly weed out those who are not among the fittest for survival.

6270. (Mr. Ram.) I wanted to ask you some questions on that point which Sir William Church has just asked you about; so that he has saved my asking you a good many; but I want just to ask you this further. Your phrase was this: that by reason of these diseases "the efficiency or otherwise of the constitution is early tested, and the candidate for life turned back if inefficient at the outset." That means that delicate children die?—Yes.

6271. In your opinion, apparently, it would seem that they should die?—I do not say that. We cannot consider it from that point of view; but Nature, no doubt, does.

6272. Then Nature takes a different view from mankind?—Yes; Nature cares nothing for the individual, but only for the type. We care for the individual; and the majority of us care nothing for the type.

6273. Do you think we are wrong in trying to save the individual?—No; I think we are right.

6274. But in your view we are acting contrary to Nature in doing so—I do not think we are acting contrary to Nature in doing so; because, for instance, when a child is born, if we leave it without food it will starve. Nature does not supply it with food, but she does not intend it to starve. Nature gives us the intelligence to supply it with food, as she gives us the intelligence to treat our sick and the affection to nurse them and care for them.

6275. In other words, Nature teaches us to thwart her own wise designs?—I do not think it is that.

6276. I will leave it at that. Is your objection to all experiments on animals that they involve some amount of suffering?—That is not the only objection; that is the main objection. There is that other objection, that it is an immoral attitude to look upon sentient living creatures which are linked to us by evolution in some way we do not understand as mere material for experiment. I think it is an immoral and demoralising attitude of mind.

6277. You think that, however great the advantage might be, we will say to a child, it would be wrong to sacrifice an animal under any circumstances?—Of course, I can see no benefit that could come to the child through it.

6278. But I was asking you if you will kindly imagine a case for a moment of great advantage to a child or, say, to many children. Granted that *ex hypothesi*, without thinking you agree with me, and that it can be obtained by sacrificing an animal, would you think it right or wrong in any circumstances to sacrifice that animal?—I think if it comes to a question of right and wrong, it would be distinctly wrong to torture an animal in order to obtain a benefit for one of our own race.

6279. And supposing it can be obtained by an experiment on an animal which is not torturing the animal, would you still think it is wrong?—Still I think it is wrong.

6280. Let me put a case to you. Supposing that there is a burning house, and there are two rooms, in one room a child and in the other room a dog—that you can save one and only one, which should you save?—Oh, of course, the child; a child is of more value.

6281. Why?—A child is of more value in the developmental scheme of Nature, apart entirely from the question of personal feeling.

6282. Now carrying that view of yours one step further, supposing there is milk being supplied to a hospital for children, and it is desired to test whether that milk is tuberculous or not, and a guinea-pig is injected with some of the milk, if the milk is not tuberculous the guinea-pig suffers nothing; if it is, it shows signs of tuberculosis, and may suffer something. Is that right or wrong?—You must admit that a guinea-pig may have a resistant power which may be sufficient to throw off the tuberculous material; in which case we should get a dangerously misleading result.

6283. I am supposing the case that the guinea-pig does not throw it off, but does show signs of tuberculosis; is it right or wrong?—I object to the whole principle. Do you know that a Swiss experimenter has tried the same thing on his own child, and has found that by administering tuberculous milk to it he was able to induce tubercle? That is the logical conclusion of this method of experimentation. And that is a result which I am all the while dreading we shall come to.

6284. I cannot see the logical conclusion of it; but, however, you have made the statement. Have you ever seen an experiment on any living animal?—Never.

6285. You suggest that it would be well to gain knowledge of the brain by examination of the brain, I think you put in your evidence, of a dead dog?—No, I do not think so.

6286. I will turn it up if you like. However, I will put it in the form of a question. Do you see any objection, if you wanted to learn something about the brain, to examine the brain of a dead dog?—Certainly not; but I think it would be misleading to suppose it would tell us anything about the brain of a living man.

6287. Supposing you want to know something about the brain of a dog, would you see any objection to that?—I have no objection to examining a dead creature.

6288. Do you think it wrong to kill an animal painlessly, so that after its death you might examine its brain?—No, since animals must be killed, I think there is no objection to killing them painlessly. We do so for food.

6289. Would you go one step further. Should you object to putting a dog under an anæsthetic from

which it cannot recover, and then examining its brain when it is wholly unconscious?—I think there is something revolting to the sense of right in the mutilation of living creatures, not for their own good, but as a mere scientific experiment.

6290. Will you point out the difference between the state of a dog which is not conscious, and never can be conscious, which must very shortly die before it recovers consciousness, and the state of a dog which is dead?—I think there is all the difference in the world.

6291. Will you point that out to me?—The fact that it is alive makes all the difference in the world.

6292. Then you said that knowledge only increased the shadows—that was the phrase you used?—I was quoting Professor Starling then. That was Professor Starling's knowledge.

6293. Yes, I think it was a quotation that you gave?—That it "intensified the black shadows of ignorance," is what he said.

6294-5. That means, does it not, that the experiments have gone to show that some of the deductions which were previously made by pathological methods only were erroneous?—No; I think it means that the experiments have gone to show that the deductions made from previous experiments were erroneous.

6296. But surely do not you think that the experiments have thrown any light upon the shadows?—No, I think that the light that was thrown was thrown by pathology, and showed up the errors which had resulted from experiments on animals.

6297. But is it your contention that experiments have never shown that previous deductions arrived at by pathological research were wrong?—I think they have not.

6298. Have you read the evidence which has been given to the Commission with regard, for instance, to the use of digitalis?—Experiments on the lower creatures could not teach us the use of drugs on human beings. Besides, digitalis acts differently in health and in disease.

6299. Have you seen the evidence that I am referring to, or read it?—No, but I would not value the evidence of its effects upon a dog a bit. For instance, you may give 15 grains of atropa, the active principle of belladonna, to a rabbit before you kill it, but $\frac{1}{15}$ of a grain will produce in a human being dangerous symptoms—uncomfortable symptoms, at all events.

6300. But if any medical men of the highest experience come here and say that they have learnt that the use of digitalis as used some few years ago was in error, and had been known in some cases even to be mischievous, you would not believe it?—I would not say that; but I should say that if they had devoted their attention to watching patients and seeing whether it benefited them, and found it did not, it would be more valuable and convincing evidence than any evidence they might get by experiments upon rabbits.

6301. The medical world have been watching a great many patients for a great many years?—Yes, and we have been gradually finding out the uses of these drugs, and the values of them. And you must remember that in the first place Nature led us to find these remedies. We did not find them in the beginning by experiments on animals.

6302. Do you think that when a new drug is discovered it is wrong in any case to test it upon animals?—I think it is not only wrong, I think it is unscientific. Because, after all the experiments you have made on animals you have to come eventually to experimenting on man, and you may find then an absolute difference in their effects.

6303. But then that may be arrived at by the process of killing two or three people?—No, you begin by the subjective method. The proper method is for students and doctors to test in small quantities upon themselves the actions of drugs—beginning with small quantities and so gradually finding out their power.

6304. As long as I am not the testing student?—But we know by analogy things which are poisonous and things which are not. We know very much by chemical analogy. And it must always come to experimenting on human beings eventually.

6305. I want to ask you a question with regard to the Society with which you are connected. Are you

aware that hundreds, I may say thousands, of very cruel operations take place daily on young animals, colts, lambs, and so forth?—Yes.

6306. Has your Society taken any action with regard to them?—We take no action of that sort. My Society exists to influence Parliament to introduce a Bill for the total abolition of vivisection.

6307. (Sir Mackenzie Chalmers.) By vivisection you mean not merely cutting, but all experiments on animals of every sort and kind?—Yes.

6308. (Colonel Lockwood.) That does not include castration, does it?—We do not go into that.

6309. You do not meddle with it?—No.

6310. That is what Mr. Ram is alluding to. (Mr. Ram.) Yes. Of course those operations are in many cases exceedingly painful?—Yes, I should suppose so.

6311. In the view of your Society, are those things which should be stopped or not?—We have not considered them. Really, we have not undertaken that aspect of the case.

6312. You have regarded the suffering which may be inflicted upon guinea-pigs and mice, and so forth, as most important, and you are anxious to get Parliamentary prohibition for it, and you have taken no action with regard to many greater operations on a very much larger number of animals, which are being performed daily?—One cannot deal with all the subjects, but I think it is a blot upon a cultured progressive science like medicine that we should mutilate and cause suffering instead of relieving suffering.

6313. Then are you taking this action with the object of purifying science, or of protecting animals?—Personally, my feeling is almost stronger about science even than it is about the animals; but that is only my personal feeling.

6314. (Colonel Lockwood.) I think you said earlier that a great deal of your evidence represented your own views?—That is so. There are only two doctors in my Society, Dr. Helen Bouchier and myself.

6315. (Sir William Collins.) You do, however, appear here as a witness, do you not, on behalf of the Parliamentary Association for the Abolition of Vivisection?—Yes.

6316. And you support that proposition of Parliamentary action with a view to abolish vivisection on the ground that vivisection is useless, I understand?—Yes.

6317. And immoral?—Yes, and unscientific.

6318. Under No. 2 of your heads of evidence you alluded to the absolute failure of experiments on animals to benefit man by establishing one important and undisputed contribution to physiology. Do you mean that no knowledge has been obtained, or that the knowledge obtained has been useless knowledge?—Later I altered my *précis*—and I took only the ductless glands and the brain, because I was scarcely prepared, although quite convinced, to go from beginning to end of the books on physiology to prove my case. As regards the ductless glands and the brain, I proved my case, I think—that experimental physiology has been able to achieve nothing. I think it is equally true of every other branch of physiology.

6319. The proposition under that second head had no relation to the ductless glands or the brain; it stands by itself—a consideration of the absolute failure of experiments on animals to benefit mankind by establishing any satisfactory or undisputed conclusions as to the proper medical treatment?—That is my belief.

6320. Did I correctly understand that you desire to revise that?—I altered it later, because, as I tell you, although I believe it, and know it can be proved, I was not prepared to prove so much.

6321. Is that because you think some knowledge may have been obtained?—No, it was not that; it was simply because it was too stupendous a subject for me to be able to prove the whole case right from beginning to end of the text books.

6322. Do I correctly understand that you wish to put your evidence on the subject of the ductless glands and the brain in a different category from that of other departments of physiology?—No, I do not; but I was prepared to prove, and I think I have succeeded in proving with regard to the brain and the ductless glands, that experimental physiology has failed absolutely to give us any knowledge of any value.

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6323. Anyway, you are not prepared to prove it with regard to vivisection generally?—I am prepared to prove a very great number of these things; but it is too wide a field for me to have been able to prove absolutely the whole case.

6324. Putting aside the argument based upon the uselessness of the knowledge obtained or the suggestion that no knowledge has been obtained, in regard to the moral question a previous witness who appeared on behalf of your Association was asked what moral principle would be violated if living animals were experimented upon for scientific purposes provided they were kept under complete anaesthesia during the whole of the experiment. What is your opinion upon that?—My opinion on that is, as I have said before, that I think it is an immoral attitude of mind which regard sentient intelligent creatures as subjects for experiment.

6325. Then, even although the experiment is completely conducted under an anaesthetic, you still think that some moral principle is violated?—Yes, I think the attitude of mind is entirely wrong.

6326. You mean that the sacrifice or the exploiting for some purpose is immoral, even though it be painless?—Yes.

6327. I think you were inclined to admit the inconsistency of one holding that opinion, but believing that animals might be sacrificed for food, or for such other purposes as were alluded to by Mr. Ram?—I do not think it is inconsistent. The majority of people believe that they are unable to live without animal food, consequently it is a question of their living or the lower animal living; and in that case they consider that they have the right to painlessly sacrifice the lower animal.

6328. Is it not a rather strong measure to suggest to Parliament to entirely abolish the utilisation of animals for scientific purposes while they are still utilised, even painfully, for food and for commercial purposes, possibly even for fashion?—I think not at all. I think medical science should be in advance of every other code of ethics.

6329. And therefore should labour under greater restrictions than other departments of human activity?—No, not labour under greater restrictions, but should go ahead and leave behind the smaller and the lesser conditions which hamper it.

6330. Do you think that that kind of moral evolution is more likely to be facilitated by a penal Act of Parliament than by leaving matters alone?—I cannot say that I was really serious about punishment for injecting sera. What I mean is that this injection of morbid principles, extracted from the blood of the lower animals, into human blood has become so serious that I think some very stringent measures should be taken to put a stop to it.

6331. But I was alluding to the abolition of vivisection by Parliamentary action?—I did not say that I would make it a legal offence.

6332. Then what is the mind of your Parliamentary Association for the Abolition of Vivisection?—I am sorry I misunderstood you—I see what you mean.

6333. How are you going to achieve the abolition of vivisection by Parliamentary action except by putting it under some penalty?—I suppose that is true. Then I am quite prepared that it should be made a penal offence to vivisection an animal.

6334. (Sir John McFadyean.) I understand that you disclaim the title of expert which was applied to you by a previous witness?—In a way I do not disclaim it. I am not recognised as an expert by the medical profession, but I consider that I am entitled, by the amount of attention and study that I have given to these subjects, to be so regarded.

6335. I want to put a question or two to bring out the weight of your authority.

(Colonel Lockwood.) The witness has already admitted that she has no practical authority; it is simply her knowledge derived from reading.

(Witness.) Yes.

(At this point Sir William Church took the Chair.)

6336. (Sir John McFadyean.) That is what I was going to ask. You have had a certain amount of opportunity to form an opinion, founded on observa-

tion, about disease in men; so that to that extent you are an authority?—I think so.

6337. But you have also asked us to adopt certain opinions of yours, which you admit have not been founded on personal observation. You have given us certain opinions with regard to diseases that possibly you have never seen, such as plague, for instance?—Yes, I think that a general principle is right or is wrong as regards every disease under the sun.

6338. But you ask us to attach some importance to your opinion, even on these human diseases, because you have devoted a great deal of time to studying the literature of them. Is that so?—Yes.

6339. That is mainly the ground on which you ask us to attach weight to your opinion?—No. I give you expert opinion to support my views. I am willing to give you further expert opinion to support my views.

6340. Coming now to the question of the practice of medicine and surgery on the lower animals and not on man, do you claim to be in any of those senses an expert?—I have had no practical experience, of course, of the treatment of animals.

6341. That, I think, you said before. But are we to take the evidence which you have given us in a general way as applying also to experiments on the lower animals for the benefit of the lower animals themselves?—Not on the observation of sick animals, certainly.

6342. But I say, in a general way, do the reasons which you have given for objecting to vivisection in order to extend knowledge with regard to human diseases, apply also to the same method of seeking for information on animal diseases?—Except that with regard to animals there is an element of reason in it, because it is less irrational to suppose that you may, by experiment on animals, learn something about the diseases of animals, than to suppose that by experiments on animals you can learn about the diseases of man. But I object to the whole principle of experimentation on living creatures.

6343. And you draw a distinction between experiments on animals intended to extend knowledge with regard to human diseases, and experiments on animals intended to increase man's power for dealing with animal disease?—I say that although both are immoral and unscientific, there is an element of reason in the one and no element of reason in the other.

6344. But I understand you to deny that experimentation on animals has added one single item of useful knowledge with regard to the means of dealing with human diseases?—Distinctly.

6345. It was a very wide statement to make?—Yes.

6346. It implies that you have read practically all the literature that there is dealing with experiments by pathologists, and physiologists, and bacteriologists. Would you ask us to believe that you are so well acquainted with the whole of that literature that we may safely accept your opinion that no useful item of knowledge has been contributed by experiments on animals?—Yes; I think we know so very little that we need not grudge attributing that little that we know to pathology and to clinical observation.

6347. But the question upon which I was seeking information was as to how much you personally know about this. It seems to me a claim that you have almost universal medical knowledge. Do not you think that you have put it a little too strongly in denying that there has been any useful addition to medical knowledge at all obtained by experimentation on animals?—I do not think so. But will you let me qualify that? If any knowledge has been obtained, it might have been obtained infinitely better by clinical observation.

6348. But I was quite content with your other answer, that you think no useful knowledge has been obtained, since you have already told me that you have devoted very great attention to the literature of the subject. But I put that question merely to lead up to this other one. Do you think that you have any right to come before this Commission and make the same statement with regard to animal diseases, that experimentation on animals has contributed absolutely nothing useful to man's power of curing and preventing animal diseases?—No, I qualify that also. I say that if it has, it might have been obtained infinitely better by clinical methods. In experimenting on animals we

may discover some truths which might have been better discovered by clinical observation; just as we might learn anatomy on living creatures, but it is unnecessary, and would be a very difficult method.

6349. But surely that is a statement that it would be very difficult to substantiate. Supposing I ask you to accept what I believe to be the general opinion of all persons of experience, that some very useful knowledge with regard to the cure and prevention of animal diseases has been obtained within the last twenty years by experimental methods, you say it would have been obtained without that experiment; but how can one be certain that it would have been obtained without it? Methods of observation were available right away back into the dark ages, and they did not discover these things. Does it not look, therefore, as if in all probability observation would not have led to the discoveries which experiment has led to?—But experiments on animals also go right back into the dark ages.

6350. Will you tell me any experiments on cattle plague going back to the dark ages?—No, I do not say that.

6351. Do you disclaim all knowledge with regard to the important contagious diseases of animals?—Yes; I know nothing about animals. I admit that I know nothing about animals. All I would be prepared to say is this: we are told that if an animal has a wound which comes into contact with the earth it is very liable to get tetanus and to die. I should say that that is a sign of two things, either that the farmyard in which the animal lives is so insanitary that it grows tetanus germs apace, or that the animal is in such a state of disease that it is far better killed off by tetanus than used for human food.

6352. But would you really seriously, if you were a veterinary surgeon, express those opinions to the farmer who called you in to prevent tetanus among his horses?—I would not, perhaps, under such circumstances; but I think that science should express those opinions. Nature has not for mischief nor for malice sown the earth with tetanus germs. The earth has been sown, if it is sown, with tetanus germs in order to test the healthy or diseased state of these cattle.

6353. But I do not think you have answered my last question, which was: Would you disclaim all knowledge with regard to important contagious diseases of animals, or would you claim to have such an amount of knowledge of these as to confer a good deal of weight on your own opinion?—I rely upon general principles.

6354. But general principles would never tell you whether the use of antirinderpest serum is of value for the prevention of rinderpest or not, would it?—I think, certainly.

6355. But I should have thought that this was one of the cases in which the method of acquiring knowledge which you are always extolling would have been of immense value, namely, observation as to whether the use of rinderpest serum stayed the progress of rinderpest among cattle exposed to the infection. Is not that the right way of observing what the value of the rinderpest serum is?—I do not think so at all. It is a wrong principle to inject morbid products from one creature into the blood of another creature.

6356. I will go into that question if you like, and I think it requires going into. Observe that this is free from some of the objections which you make regarding the use of animal serum on human beings. Will you tell me what is the morbid material in ox serum injected into an ox? What do you mean by morbid?—I think that science does not know what it means by morbid.

6357. I am quite certain that I do not know what you mean by it, but I think it is right that you should explain it?—Suppose we say, then, the products of the micro-organisms, not the micro-organisms themselves nor their toxins, but the products of the micro-organisms. May I read you a short extract?

6358. I would much rather you answered me specifically?—I think it would be better to answer you by giving you the opinion of an expert rather than my own opinion.

6359. Will you tell me who is the expert who has given an opinion on the antirinderpest serum?—No, it is not given upon that.

6360. Probably the opinion you are going to read me does not apply to the particular case, which is the use

of ox serum?—Do you mean ox serum upon another ox?

6361. Ox serum upon another ox; that is how antirinderpest serum is obtained. I asked you in what sense it was true that such blood contains anything morbid. Does it mean capable of causing disease?—Capable of results of which we have no conception—not necessarily immediate results.

6362. I believe you are quite right in saying that we have no conception of them; but the great majority, I think—all, in fact, of those who have had an opportunity of observing—deny that there are any disease-producing powers in this serum. But since you allege that there are such disease-producing powers in the antirinderpest serum, I must ask you to quote the authority?—I should be extremely sorry to eat part of an ox which had been injected with such serum, and I think there is very great danger in injecting cattle which are used for food with these sera.

6363. But you must allow me to say that there does not seem to be any value in your opinion with regard to this question. I mean that it does not seem to advance at all what we are inquiring after, to tell us that you are satisfied that it must be dangerous, if you can give us no evidence whatever that it is dangerous. Will you take it from me that hundreds of thousands of animals (of some of which you have probably eaten part) have had this antirinderpest serum inoculated into their bodies, and there is a large literature on the subject? Could you bring anybody here to furnish an atom of evidence to the Commission that such serum is injurious?—No, I do not say that I could; but I do not consider that these things have been used long enough for us to be able to test their far-reaching results.

6364. But have you any knowledge of the extent to which antirinderpest serum has been used, and for how long?—No, but I think if one of these sera is injurious it is rational to suppose that another is, and that if the principle of serum-therapy is wrong, it is wrong in every instance.

6365. Is not the only safe way of arriving at an opinion as to whether it is useful or not to make observations on animals which have been treated with the serum? Why should you discredit observation in this single case?—I think it is extremely difficult for us to judge of the results. I say on the face of it that it must be wrong to inject the diseased blood of one animal into another healthy animal, and nothing can make it right.

6366. Whether it is right on the face of it or not, you are not prepared to deny, are you, that the antirinderpest serum has been of great value in preventing rinderpest?—From analogy I should say—from our experience with other sera, that in a very short time it will be found to be a failure, just as Koch's tuberculin and so many of these other sera have been proved failures.

6367. But it must be remembered that the motive for using antirinderpest serum and such remedies on animals is a purely economic motive, and you think that farmers and cattle owners have been absolutely deluded—they are mostly very shrewd men—when they consider that this method of treatment has been most successful, and has saved millions of money in some countries. You still think they are simply under a delusion?—I think that the bubble will explode, as all the other bubbles have exploded, when sufficient statistics have been collected. And I also object to the immunity that it confers—that is the very principle.

6368. You are a great believer in following the course of Nature, are you not?—I am, very great; because not all our scientists could heal a pin-scratch were Nature not to set in operation her law of repair.

6369. Now, supposing that I go back for a moment to this same antirinderpest serum, do you know whether it is a fact that Nature cures an attack of rinderpest naturally contracted, by causing the development in the patient's body of something which is destructive to the germ of rinderpest?—No, that chemical theory of antitoxins is quite exploded. The latest theory is that it is the living cell which resists the poison, not that there are a toxin and an antitoxin, which are antidotal to one another.

6370. It might serve my purpose just as well to put it this way: that during recovery from a natural attack of rinderpest the animal acquires powers of resistance that it did not possess before, or possesses these in a

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heightened degree?—The powers of resistance are, of course, along the special lines which are necessary to throw off rinderpest.

6371. You do not deny that when an animal has recovered from rinderpest it is scarcely possible to kill it with rinderpest afterwards?—That, of course, is probable.

6372. Do you deny that the protective properties which it possesses can be transferred to another animal by transfusing the blood from one animal into another?—I think it is absurd to suppose that that can be true.

6373. Then you think that all those who hold that that is true, and who have had endless opportunities of observing whether it is true or not, are mistaken?—No, I do not think that. I think it produces what they call immunity, which is simply that it accustoms the creature—produces a vicious tolerance—to the poison of rinderpest.

6374. But the farmer does not care anything about that?—But we do who have to eat his cattle. It is very important to us that we should follow out natural methods, and should get animals in a high state of health.

6375. Then would you positively seek to restrain farmers from using these remedies, which they believe to be of great economic value?—Certainly I would. I think that nature knows best whether these creatures are fit to live or whether they are unfit to live.

6376. You spoke about hypnotic influence, to which human beings are sometimes unconsciously subjected by the physician when he puts a hypodermic needle into them?—Yes; I have seen patients accustomed to take a morphia dose at night by means of a hypodermic injection sleep equally well when they had a hypodermic injection of water.

6377. Do you think the same effect is produced on animals when a veterinary surgeon injects them—that it has not an immediate effect then?—I know nothing about hypnotic power over animals, but I believe from what I have heard from hypnotists that it is possible to hypnotise animals.

6378. In answer to a question put to you on the previous occasion, at No. 5301, you said that if a certain observer had proved by his experiments some hypothesis absolutely different from the early observers, he would yet not have proved it. That is a somewhat enigmatic statement?—I know. I added, because the earlier observers would have already “proved” the contrary.

6379. I think what you meant was that the very fact that one experimenter arrives at a certain conclusion as the result of his experiments, and another experimenter arrives at a different conclusion with regard to the same matter as the result of a second set of experiments—that fact of divergence of view proves that that method of seeking to acquire knowledge is useless?—Not the divergence of view, but the divergence of fact. One fact nullifies another.

6380. But what is the fact here? A certain experiment is performed in order to throw light upon some obscure point, that is to say, to try to find out whether a particular opinion is right or wrong, and two different observers draw different deductions from their several experiments?—If you will allow me to say so, two different observers produce different effects on animals by the same operation. For example, with regard to the functions of the thyroid and parathyroid glands, one set of observers observe that removing the thyroid and parathyroid glands causes tetanus and myxedema; another set of observers find it causes neither tetanus nor myxedema, and I consider that the one set of experiments nullify the others; so that if the first man had proved his case and the second man had proved his case nothing, after all, would have been proved.

6381. You would extend that to all experiments; you think from the fact that with regard to the function of a particular gland different experimenters have appeared to produce different effects in their experiments, therefore that method of experimentation is entirely discredited?—I think the varying results depend upon the difference between one animal and another—I mean not only the difference between different species of animals—but the idiosyncrasy of a particular animal. One cat has nine lives, another cat has only seven; a rat will die under conditions that a rabbit will survive. All these conclusions are so confusing that they simply hamper medical science.

6382. Supposing now that two observers watching the same patient, or making observations on the same disease, arrive at diametrically opposite conclusions with regard to some important point, does that discredit observation?—Not in one or two cases. But these experiments take place year after year, decade after decade, and the results are equally conflicting in all cases.

6383. But surely observation has been going on for longer than that. I should have thought that observation had led to very different conclusions?—Observation is gradually giving us some knowledge. It is the only method by which we arrive at knowledge. In an experiment you produce an absolutely artificial state of things. It is not the same thing as in Nature, and it is not the same thing as the morbid condition that occurs in disease.

6384. I do not know that you quite explained, in answer to Sir William Church's question, why you refused to accept the explanation of the increased number of diphtheria cases with paralysis which was put forward in the extract you read to us, namely, that the use of antitoxin saves the lives of a good many children, who would otherwise have died, and that the apparent increase in the cases of paralysis after diphtheria is due to that circumstance?—I think it is too far-fetched a hypothesis for anyone to admit.

6385. But will you tell us why it is far-fetched when I suggest that it is true on this account. Is it not the poison generated by the diphtheria bacillus which kills in diphtheria?—Yes, let us admit that. It is the diphtheric poison.

6386. It is the diphtheritic poison that causes the paralysis in those children that have had no antitoxin serum?—Bosanquet says that the antitoxic serum has no effect upon paralysis.

6387. I am not asking you that question. I suppose you admit that a considerable number of children who acquire diphtheria and are not treated by the serum at all become paralysed afterwards?—Certainly.

6388. Is that ascribable to the toxin in their bodies?—We will allow that it is, certainly.

6389. Then it is the toxin that kills also, as you have already admitted?—Yes.

6390. So that presumably the antidiphtheria serum, if it saves any lives at all, would almost necessarily cause the apparent increase in those cases with paralysis?—Not at all, because all cases of diphtheria do not cause paralysis. It is not necessarily the natural course of diphtheria.

6391. Do not you think that upon the whole it is the worst cases that get it?—No, I do not think that is so. I think you will find from the text books that paralysis often occurs in quite slight attacks, and I will tell you what this means. It means that just because the tonsils are not throwing the poison out of the blood the poison acts upon the heart muscles and upon the other muscles. And I think antitoxin also throws the poison back into the blood and prevents the tonsils from excreting it, so that it apparently only benefits the patient.

6392. In answer to a question put to you on the previous occasion, Question 5369, I see you say: “For instance, I say this, as I was saying about Dr. Paton, if he has shown that in conferring the immunity of a creature we also confer the susceptibility of the creature, how can we say that vaccination has not given to children the great susceptibility to tuberculosis in calves?” What suggested that to your mind? Have you ever read anywhere that the susceptibility of the human race to tuberculosis has been increased by vaccination?—No. Dr. Paton says that when he was giving, even by the mouth, the serum of horses and sheep, he conferred upon the human patient the great susceptibility of horses and sheep to influenza and catarrh. I think that that is evidence that we are able to transfer the susceptibility of animals to human beings. Consequently, as calves are well known to be very susceptible to tuberculosis, how do we know that we have not conferred upon the human infant, in the tender infant years, the great susceptibility of calves to tuberculosis?

6393. I was going to ask you about the susceptibility of these lower animals to different diseases. Who is Dr. Paton?—Dr. Montmerie Paton's book, “New Serum Therapy,” is considered a very important contribution to the science of serum therapy.

6394. Did I correctly understand you to say that he said that he had conferred a susceptibility to catarrh?—And influenza.

6395. Of what animal?—Of sheep and horses.

6396. Would you like the Commission to believe that sheep are very susceptible to catarrh?—I do not know. I give it on the authority of Dr. Montgomerie Paton, who is considered an authority.

6397. Would it surprise you if I were to tell you, having had a good deal of opportunity to observe, that sheep are extraordinarily insusceptible to catarrhal complaints of the air passages—that it is almost unknown amongst them? You would not deny that?—I would not deny it. The observation was not my own. It was quoted from a man who is an authority.

6398. You ask us to accept it?—I ask you to accept his statement.

6399. Do you desire us to believe that the horse which is used for the manufacture of antidiphtheritic serum is remarkably susceptible to diphtheria?—Do you say that it is?

6400. I want to know whether you say that it is?—I know this, which is an absurd anomaly. Professor Ehrlich, who is considered a great authority upon the question of sera, states that from 20 to 30 per cent. of healthy horses have in their blood diphtheric antitoxin. Now, how can diphtheric antitoxin be an antidote which the system manufactures to neutralise diphtheria if it occurs in from 20 to 30 per cent. of healthy horses?

6401. Who is your authority for suggesting that calves are more susceptible to tuberculosis than human beings?—I cannot give you my authority for that.

6402. Could you find any authority for it?—No.

6403. But you have implied here that it is a fact?—Surely it is proved by the susceptibility of calves to tuberculosis which has been shown by this recent Tuberculosis Commission. They found that the calves readily took tuberculosis by feeding with tuberculous milk and with the sputum of tuberculous patients. Surely that shows that they are very susceptible to tuberculosis.

6404. Do you profess to have great knowledge of the Report of the Royal Commission on Tuberculosis?—I do not profess to have great knowledge, but I have a very fair knowledge.

6405. Would you like this Commission to believe that any calves were killed by tuberculosis by feeding with sputum?—I do not say killed.

6406. That they were made ill?—Tuberculosis was transferred.

6407. Surely that does not prove that calves are more susceptible to tuberculosis than are human beings. I suppose about 70,000 people die from tuberculosis in this country annually, and perhaps ten times that number contract it without dying?—I think you would be quite unable to say certainly that that great death rate from tuberculosis is not very much the result of vaccination having conferred the susceptibility of the calf to the human infant.

6408. Then what accounts for the susceptibility of calves, because they are not vaccinated with anything?—It may be entirely natural to them.

6409. Now, is not that rather far-fetched—that you employ one explanation of the susceptibility of calves and another of the susceptibility of human beings?—Not at all. From my experience of the way in which cows and calves are kept by farmers, I do not think it is any wonder at all that they are most susceptible to tuberculosis. You must remember that in the case of human beings we now give them good ventilation, and have improved all their conditions. This should have put a stop to tuberculosis; and we are not putting a stop to tuberculosis.

6410. So that you do not wish to modify the opinion implied in your previous evidence, that calves are more susceptible to tuberculosis than human beings?—I do not wish to do so.

6411. But you cannot bring before us any evidence showing that?—No, I can bring no direct evidence.

6412. In answer to question 5372, on the previous day, you spoke about the torture of innocent creatures by men, vicariously to escape the consequences of their own trespasses against the laws of health, and elsewhere you speak about published cruelty. Now, will

you cite to the Commission any experiment performed within the limits of these islands during the last ten years that could fairly be described, without exaggeration, as torture of the animal?—Certainly. I think that these experiments can be described only as torture. Miss A. Kenealy, L.R.C.P. 12 Mar. 1907.

6413. What do you mean by torture?—An extreme amount of pain.

6414. And you say that they would all be correctly described as torture?—No, I do not say that they would all be correctly described as torture, because I think in some cases, no doubt, the anæsthesia is complete; but I think that in a great number of prolonged cases of operation the anæsthesia is not complete, and I imagine that nothing within the imagination of man can picture the terrible amount of suffering which is then induced.

6415. But it would be the easier for you if there are so many cases to cite one or two specific instances which you think could, without exaggeration, be described as torture?—I have not taken up the question of pain—that is so accepted.

6416. But the use of the word torture implies, as you correctly say, severe pain?—Yes, severe pain.

6417. But you cannot mention any specific case?—I do not wish to mention any specific case. I have not prepared any specific case. But I think our Chairman produced specific cases before you, and you will find that a great number of other witnesses will also produce specific cases of pain.

6418. But torture was your expression?—Specific cases of torture, then.

6419. You admit a distinction between pain and torture?—I do, certainly. I think that no pain is equal to the torture of vivisection.

6420. No pain?—No pain can be equal to the torture of a knife among the living tissues.

6421. But surely the pain caused by similar lesions and alteration of issues without anæsthetics must be much greater than the pain experienced under anæsthetics?—I think not. I think the whole thing, the fright and terror, the horror of the creature finding itself at the mercy of a merciless person with a knife, is appalling.

6422. Do you think that the administration of anæsthetics aggravates the pain really?—No.

6423. But you said that you could not imagine any pain like that of these experiments, including those where an anæsthetic is used?—If the creature were absolutely anæsthetised of course there would be no pain; but the question is that these creatures are not fully anæsthetised.

6424. The question is that?—The point is that these creatures are not anæsthetised.

6425. Do you wish to give evidence on that point?—I am not giving evidence on the subject of pain, because I have not prepared it; but you will find before these proceedings are over that you will not be able to doubt it.

6426. I have one or two more questions which I want to ask you. In answer to Question 5407, on the previous day you said, "I think it has been proved to be quite impossible to thoroughly anæsthetise," you were speaking of animals?—I meant for such long periods of time.

6427. How long is that?—Well, I should be sorry to state, because I have not fully gone into the question of anæsthetics.

6428. But you say that you think it has been proved?—I think it has been proved from the evidence.

6429. Would you mind telling us who has proved it?—Sir Thornley Stoker, for example.

6430. But he admitted that he had not attempted to anæsthetise more than about a score of dogs altogether. Do you think that proves it?—No, I do not think that proves it; but I think there is much evidence to show that dogs, for example, are most susceptible to chloroform. I do not think it is questioned. That was brought out before the last Commission. And for that reason I think it is certainly impossible to keep dogs absolutely under anæsthesia during these long experiments. There is this also to be said, that in an operation upon a human being it requires one person's whole attention to see that the patient is properly anæsthetised; whereas Professor Starling admitted that it is the laboratory boy who anæsthetises the creatures. That seems to me to be permitting the

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laboratory boy to be the judge of whether the creature is absolutely under anæsthesia. Besides, why then do they give curare to keep the creature quiet? In an operation upon a human being there are no movements. If there are movements during the operation the surgeon at once stops his operation and looks at the chloroformist (I have seen it scores of times, hundreds of times) to imply that more chloroform must be given. Curare cannot be necessary if there is complete anæsthesia.

6431. But that is hardly dealing with the question of the impossibility of anæsthetising?—I think so—the impossibility of completely anæsthetising.

6432. So that you do not wish to modify the answer here that you think it has been proved to be impossible?—I do not wish to modify my answer a bit.

6433. And you can cite no other authority than Sir Thornley Stoker?—And my own experience in operations.

6434. But this is with regard to dogs?—But the same must be true of human beings.

6435. Is that so; that what is true of human beings must be true of dogs?—Not necessarily so.

6436. But a minute ago you said that it was?—I think it applies to anæsthetics—that is the practical question.

6437. I think so too. But supposing there should come before the Commission a witness who has chloroformed or anæsthetised a thousand dogs (anæsthetised them for their own good most of them) and he tells us that he dissents from the opinion that you hold, do you think the Commission would be right in adopting his opinion rather than yours?—I think if the Commission were to ask him whether the laboratory boy is a suitable person to anæsthetise a dog he would tell you no. For example, Professor Hobday in his book on the surgery of dogs and cats says that one person must give his whole attention to the anæsthetising of the animals.

6438. I am sorry to interrupt you, but the question was not as to whether any particular cases were anæsthetised, but as to the impossibility of anæsthetising?—What I meant was, not the impossibility of anæsthetising them, but the impossibility of keeping them for hours under anæsthetisation, seeing that dogs are peculiarly sensitive to the effect of chloroform.

6439. What is the principal danger in the case of human beings in the administration of chloroform?—The principal danger is that it stops respiration.

6440. That is to say the possible danger is that you may give too much?—Precisely.

6441. But you do not object to a dog getting too much, when it is under the vivisector's hands; you would desire it to be killed immediately?—No, I do not object to it being given too much. I should be thankful, because it would put a stop to its sufferings. But the vivisector, the operator, the experimenter, has not that same feeling.

6442. But you have just told me that the main danger of anæsthetising in human beings (presumably a thing that is likely to happen if it is not done carefully) is that too much is given?—Yes, that too much is given.

6443. So that probably that is also a danger when the laboratory boy does it?—Not at all, because the experimenter would be very careful that the laboratory boy did not spoil his experiment by giving too much.

6444. Do not you think that the experimenter is also anxious to save the animal pain?—I do, certainly I do. I do not accuse anybody of wanton cruelty, but I think that in their zeal they over-value these experiments—they have been brought up to over-value them. We are told that children who have been brought up in narrow streets become so short-sighted that they only see from one side to the other. In the same way, I think, these experimental physiologists become so mentally short-sighted that they can only see the value of their experiment. Consequently, I think, they are apt to lose sight of the pain of the animal.

6445. They may be mentally short-sighted, but for

the most part they are very intelligent men, and they tell us that it would be no advantage to them, but in most cases a distinct disadvantage, if the dog or other animal were not anæsthetised?—Then why do they give curare?

6446. Do you ask us to believe that in the great majority of cases curare is administered at the present time to dogs under experiment?—In a number of experiments, I have noticed that curare is given—to stop muscular movement.

6447. The question is, in what proportion of cases at the present time, say, during the last five years, has curare been administered?—I am not prepared to say in what proportion; but certainly it is given.

6448. You imply that it is the custom?—I believe it is common.

6449. Can you bring any evidence before the Commission; you can take time to look it up and furnish the Commission with evidence to show that curare is habitually or customarily employed?—In those very cruel experiments of Dr. Cushny curare and paraldehyd were employed.

6450. That is not the question. The question is whether it is customary, that is to say, the rule, to give curare at the present time in anæsthetising dogs?—But whether it is the custom or not—supposing it were only the exception, I take exception to that exception.

6451. But that is not the question; whether you take exception to it or not has nothing to do with my question?—I am not prepared to tell you, because I have not prepared statistics of the number of cases in which curare is given; but curare is given, and is given to a very great extent, I think.

6452. Can you give us any evidence of that?—I give you Dr. Cushny's, in which those most cruel experiments were performed with curare and paraldehyd—I should say some of the most agonising experiments that it is possible for a wretched creature to be subjected to.

6453. I understood you to introduce the question of administering curare as evidence that vivisectors had no confidence in their ability to anæsthetise dogs?—That they have no confidence in the ability of the laboratory boy to keep them properly anæsthetised.

6454. Therefore you must have intended to suggest that as a rule they administer curare?—I would ask the Commissioners to be good enough to put that question to these experimenters—why do they give curare?

6455. But one of the Commissioners takes the liberty of asking you to provide the Commission with evidence showing that curare is as commonly used as what you said implies, and we will allow you time to look it up?—I will look it up, if you wish it to be ascertained.

6456. It can be put in your answer to this question?—Yes.*

6457. (Dr. Gaskell.) Can you tell us in the evidence of Professor Cushny where he says that it was curare and paraldehyd? I can only find morphia and paraldehyd?—The extract was taken from the "British Medical Journal," I think I am right in saying curare was given. I may be mistaken.

6458. It is morphia?—I am sorry; I thought it was curare.

6459. I do not think there is any question of curare in Dr. Cushny's evidence with regard to that?—I am sorry. But the creatures were not anæsthetised.

6460. (Sir John McFadyean.) Still, you will be able to find a good many other cases?—Oh, certainly.

6461. I see that you deny man's right to do what you call exploiting animals. What exactly do you mean by exploiting animals?—What I have said so often here, that I object to regarding sentient living creatures as subjects for experiment. That is exploiting them.

6462. By that do you mean causing them pain or inconvenience?—For example, to cut up a frog as though it were a turnip—a struggling, living frog as though it were a turnip. I think that is a blot on medical science. And there is this to be said, which I

* Miss Kencahy subsequently wrote: "In the two 'Journals of Physiology,' from which I have been quoting, I find in the number for December, 1904, that curare was given in one out of six vivisection experiments, and in the number for August, 1906, curare was given in one out of seven vivisection experiments described there."

do not think has been considered. We are told that a large number of these frogs are pithed, but it is admitted that a number of these frogs are not pithed. And physiologists tell us that if you apply acid to a frog which has had its whole brain removed it will move its legs and try to brush away the acid; that it will perform many acts, even catching its food and swimming, after its brain has been removed. I question, therefore, whether we have proof that the power of sensibility in the frog lies in the brain.

6463. That is rather remote from my question?—I think not.

6464. Which is the right to interfere with what might be called Nature in respect to animal freedom. We have the right, I suppose, to interfere with Nature in regard to the freedom of animals?—It is a right that we have assumed.

6465. What is the justification for it?—The justification for it is the belief that human beings cannot live without animal food, or cannot live to the utmost of their power without animal food.

6466. Would that justify the cutting off of the tails of some three or four million lambs annually in this country?—But I do not approve of that. I do not think two wrongs make a right.

6467. No, but do you disapprove of it strongly?—I believe that these things are dying out with the spread of civilisation and humanity. For instance, the cropping of bulldogs' ears is now discountenanced.

6468. But take this one thing, the cutting of lambs' tails, do you think that is dying out?—I do not know anything about it.

6469. Would you suggest that it ought to be prohibited by law?—I do not know anything about it; but I say that medical science should not be in the van of committing cruelties on animals.

6470. You represent the crusade against vivisection as being mainly inspired by a desire to keep medical science pure and to elevate it, is that so?—Very largely that, and also to advance it.

6471. Do the bulk of those who take part in this agitation belong to the medical profession?—I do not think the bulk do. Medical persons, you know, are even more conventional than others; they seldom take original views. Besides, the great majority of medical men, when spoken to upon the subject, say: "Really, I have never seen a vivisection—I do not know; I do not think men are cruel." That is all they can say. The majority of them have not seen vivisections.

6472. Then is it not curious that it should be mainly a layman's movement if it is one intended to benefit medical science?—I do not think so. I think that reforms generally come from outside a profession. Because people are bound by the traditions and conventions of the profession to which they belong. I believe it has happened to cut in the street a doctor who was a member of an anti-vivisection society.

6473. You say that man has a moral right to kill animals?—I do not say so, but custom assumes it.

6474. But you did say last time, in answer to Question 5384, that man had a moral right to kill them for food?—I will not affirm that we have a moral right.

6475. You want to take that answer back?—No, I do not really. It is an assumed right.

6476. Vivisectioners assume a right to perform certain experiments in order to extend our knowledge with regard to disease—that is an assumed right, too?—I presume to assume that vivisectioners have done nothing but hamper medical science, because they tell us only of what is valueless to us—the peritonea of rabbits, the stomachs of dogs, and the brains of apes. We want to know about human beings.

6477. But you did not say that the killing of animals for food was an assumed right; you said it was a moral right?—It is a right that I am not prepared to contest.

6478. That is to say you admit that man has a moral right to kill animals for food?—Painlessly. I think that with evolution and development we shall get a higher moral sense, and shall realise that we have no right to kill animals for food.

6479. You also said on the previous day that you believed that animals are painlessly killed for food purposes?—I know nothing about it. But if they are not painlessly killed, I think it is time we should see that they are painlessly killed.

6480. Supposing it to be a fact that all over this country sheep are, as a rule, painfully killed, that is to say, have their throats cut in full consciousness, would you not admit that the pain inflicted on animals in that way is a hundredfold greater than all the pain that is inflicted by so-called vivisection?—No, I would not admit that. And I would say that it is an entirely different thing for a butcher, who is a boor, to kill a creature in that way, and for a man of science, a man of cultivation, a man of enlightenment, a man who belongs to a humane profession, to torture creatures or to occasion them pain.

6481. You would not admit that important considerations in the case are, first, what is the object of inflicting pain, and, secondly, whether pain is avoidable or not. Do not you think it deserves the name of cruelty, and ought to be put down by law if it is unnecessary to inflict pain on animals in killing them?—Certainly I think so. But I think it is not so great a blot upon our civilisation when these things are done by boors. For example, if a schoolboy throws a stone at a frog, or a boor kills a frog with his boot, it is not the same thing as when men who are leading thought, men who are responsible members of society, inflict torture upon sentient intelligent creatures.

6482. So that your feelings when you eat a piece of mutton are not at all wrung?—Yes, they are.

6483. But you did not know till I told you that killing sheep involved any pain, because you told us on the previous day that they were painlessly killed?—It is absolutely against my principles. It is only under compulsion that I eat meat. I tried vegetarianism for eighteen months, and found that I could not do my work without meat.

6484. That hardly justifies you in assuming a right to inflict pain on animals. None of us are necessary?—I am sorry to hear there is pain. I thought there was not pain. But I do not think that that in any way justifies the pain inflicted by vivisection. It has been said that because there is pain in Nature it is no harm for vivisectioners to increase that pain. But because there is pain in one way is a special reason for not increasing pain in others.

6485. Then would you desire that legislation should be introduced to compel butchers to administer an anæsthetic to all the sheep in killing them?—To kill them painlessly?

6486. Yes?—I would have them killed painlessly. I think that all people of ordinary humane sense are of that opinion.

6487. You and some other witnesses have represented that, owing to the fact that vivisection is practised by physiologists and pathologists, the means of acquiring knowledge by observation has been greatly neglected?—Yes.

6488. It does not appear to me why that should be so. Do you know how many medical practitioners there are in this country?—I do.

6489. How many?—I say I do; but I cannot give you the figures.

6490. About how many?—I have not the remotest idea in figures.

6491. I think it is about 30,000?—Yes, a large number.

6492. Do you know how many of them altogether are engaged in vivisection in this country?—Quite a small number, but they are the men who are supposed to lead thought and who are leading thought. The ordinary medical practitioner is not a man who makes discoveries in science. He is not a man of practical value to medical science. He is for the most part a rule of thumb person who believes what medical scientists tell him.

6493. It requires great courage on your part to say so?—I do not consider it a reflection on the medical profession.

6494. I am bound to say that I do not agree with you?—A man in active practice has no time to make discoveries. He expects the medical scientists to do that for him, and I think they are attempting to do so by devoting their talents in a wrong direction.

6495. But I believe, if you take only the Medical Faculties of the schools and universities, it is only a minority of them who are engaged in vivisection?—Yes, I think so, but then my view is that the majority of the medical profession are misled by those men.

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6496. But the majority are not concerned with vivisection, and therefore their powers of observation ought not to be thwarted in any way?—For example, every medical student learns his physiology from what is called a Text Book of Human Physiology, and if you look into it, it is a text book mainly of the physiology of cats, and rabbits, and dogs.

6497. That is one of your reasons for objecting to vivisection?—Exactly.

6498. But that reason drops to the ground in the case of veterinary physiology?—I do not think we are dealing now with veterinary physiology.

6499. Yes; veterinary surgeons are very much concerned in this inquiry, too, because if knowledge with regard to animal diseases may be extended by experiment on the lower animals, they naturally do not wish that channel of inquiry to be stopped. Do you think that it is advisable that it should be stopped?—Yes, I think the same thing is true in both cases; that you cannot learn physiology by mutilating creatures, because when you mutilate them, when you anaesthetise them, when you narcotise them, they are under entirely abnormal conditions.

6500. When you were here last you expressed your disbelief in the common view that the tubercle bacillus is the cause of tubercle?—No, I do not think I expressed my disbelief; I said that it had not been proved.

6501. You said that you were not satisfied that it was so?—Not only that I am not satisfied, but that medical scientists themselves are not satisfied.

6502. Will you tell me one medical scientist who is not satisfied?—Dr. Granville Bantock, a man of thought and originality.

6503. Is he coming here?—Yes.

6504. We will go more fully into it with him, then. But the fact that you say that you are not satisfied implies that you have a good deal of knowledge regarding the evidence on which the belief is founded. Would you tell the Commission what are the defects in the chain of evidence which Koch produced to prove that the tubercle bacillus is the cause of tuberculosis?—I think it is quite impossible to prove that it is the bacillus itself. It may be some product of the bacillus, it may be some potency that we do not understand, and independent of the bacillus, although the bacillus is present.

6505. Do you really profess to be well acquainted with what are commonly called Koch's postulates—that is to say, what is necessary to prove that a particular organism is the cause of a particular disease?—May I ask whether you do not consider it rather rash to quote a man like Dr. Koch, who has been so frequently proved wrong?

6506. I am not the witness, but I will answer that and say that I have a great admiration for Professor Koch, and I think he is a great authority?—But still, you cannot help admitting that he has been frequently proved wrong.

6507. That is beside the question. My question was whether you profess to be well acquainted with what are commonly called Koch's postulates?—I think that one case in which a disease has been transmitted without the intervention of a bacillus—when it has been proved that there is no bacillus—is sufficient to prove that a bacillus is not necessary.

6508. Will you tell me what case that was; will you give us the reference?—I will leave it to Dr. Granville Bantock, who knows this aspect of the subject better than I do.

6509. But you said that you were not satisfied?—Yes.

6510. That implies an amount of knowledge necessary to detect a defect in the evidence produced by Koch to prove that the bacillus was the cause of consumption. I will put it to you in this way: Supposing I were in possession of what is called the tubercle bacillus in artificial culture, and I can show that I can produce any number of cases of tuberculosis that I desire by injecting some of this culture under the skin (as has been done in these experiments which you refer to for the Royal Commission on Tuberculosis), will you show me where the error arises in concluding that the bacillus is the cause of the disease which follows?—It may be a product of the bacillus, and not the bacillus itself.

6510A. That is all you mean?—It may be something

clinging to the bacillus; it may be some potency of the bacillus which we do not understand. But it is simply a question of detail. There is something that causes disease; whether it is a bacillus or whether it is a product of the bacillus, or something clinging to the bacillus, is a mere detail.

6511. Surely it is of importance, if it be a detail, whether it is a bacillus or something else?—There is a cause; we know there must be a cause.

6512. (Sir Mackenzie Chalmers.) I have not very much that I wish to ask you. How long did you practise medicine yourself?—For eight or nine years.

6513. As a physician or surgeon, or as a general practitioner?—As a general practitioner. Of course, women do not attend men; they attend only women and children.

6514. Where were you in practice?—In Watford, and I had a consulting practice in Henrietta Street, Cavendish Square.

6515. When did you abandon medical practice?—About six or seven years ago. I had a very bad attack of diphtheria, which incapacitated me for a while.

6516. If I understood you aright, you did not interest yourself in any experiments on animals until after you abandoned actual practice?—No, I took it for granted that there were no cruelties practised. I accepted the conventional view of the profession that there were no cruelties practised. About two or three years ago I was led to look into the subject, and found the appalling lengths to which vivisection has gone, and the appalling way it is increasing, and that cruelties are practised.

6517. While you were in actual practice it did not occur to you?—No, just as it does not occur to the majority of medical persons.

6518. You would agree, I suppose, that the leaders in the medical profession are certainly distinguished by humanity and kindness?—Practising doctors are, I admit.

6519. But the opinion of the leaders of the profession is strongly opposed to your view, is it not?—Yes.

6520. How do you account for that; what is the cause of the conflict between you?—I think it is this: Huxley said "All science begins as a heresy and ends as a superstition." I think that vivisection has become, not a superstition, but a fetish, and it is now a fetish that is hampering and restricting the progress of medicine.

6521. Is it not extraordinary that men of this high ability and this high scientific knowledge should become a prey to a fetish?—It is astonishing. But everybody knows that this fetish, belief in the miraculous properties of blood, for example, has characterised all nations; it is one of the most difficult creeds to die. This belief inspired the old philosophers looking for the philosopher's stone, inspired the massacres of Benin. Every race is misled by this fetish belief in the miraculous properties of blood. It is the same in religion—the mystical properties of blood to save.

6522. That is your explanation. But you have come to the conclusion, and your Society have come to the conclusion, that experimenters are morally wrong?—Yes.

6523. At any rate there are a great many people who think that they are morally right, and that it is only justifiable to administer drugs to a human being which have been first tried on animals. You admit that that is held by a great many people?—Yes, I admit that it is, but the truth is that the experiment must always come in the end to be applied to a human being. And why not begin with that?

6524. All I wanted to get at was this. You have at any rate a conflict of consciences there?—Yes, certainly.

6525. But by what standard are we as Commissioners to judge which conscience is to be obeyed?—I think you will find that in all professions there are narrow restrictions and traditions which govern its members detrimentally, and that very often persons outside the profession are able to take a broader and really more progressive view than those within it.

6526. Then what becomes of the standard you would set up? What is the standard by which you are to say it is wrong; is there any standard by which we can measure?—Yes, I think the human standard instead of the technical standard.

6527. I do not quite see what you mean by the human standard. What is the test of it? One branch of humanity holds it right, the other branch of humanity holds it wrong. Where are we to get any test? Is individual feeling to be your test, the conscience, or what?—I think that the reason vivisection is upheld by the members of the medical profession is simply due to class prejudice, the sort of professional prejudice which has over and over again been known to stand in the way of progress.

6528. But then a great many people outside the profession say that we are justified in using the lower animals, with, of course, proper regard to humanity, for any purpose useful to man. Where do you draw the distinction between right and wrong there?—It seems to me that we have only to take the internal evidence.

6529. Our own consciences?—I am not speaking only of that, but of the practical results.

6530. That is to say you would not think that vivisection was wrong if you were satisfied that useful results followed?—Yes, I would not depart from that standard at all.

6531. Then I want really to get at it, and to see what your standard is?—Would you please put the question again?

6532. What are we to take as the standard of what is right and wrong in dealing with the lower animals?—It is difficult to say what we are to take as the standard. I would say the feeling of large-hearted, intelligent, progressive persons, rather than of persons who are governed by the traditions of a class and by the fetishes of a class.

6533. Would not the same apply to the killing and wounding of animals in sport?—Of course, I do not like that, but I do not think there is anything comparable to the horror and pain of vivisection. See how we all shun the dentist! What must it be to be absolutely in the power of a man who clamps one to a board and cuts up the living tissues?

6534. You know that there are strict restrictions under the Act?—Take the frogs. Nobody gives chloroform, or professes to give chloroform, to a frog. During these proceedings one of the Commissioners asked the Inspector who was giving evidence "Why should not goats be used?" and the Inspector, who had been showing that there was no pain in vivisection, said, "No, I am fond of goats." Although there was no pain in vivisection he was fond of goats and would not allow them to be vivisected. Then the Commissioner said, "And I am fond of dogs." I am not a Commissioner and I am not an Inspector, but may I not urge the plea that I am fond of frogs?

6535. Then practically it comes to this, that it is a matter of individual sentiment, and nothing else?—I do not think it is a question of sentiment; I think it is abhorrent to the humane sense to take a living frog and pin it out on a board, and cut into it.

6536. Is it worse than taking a living fish and playing it on a hook?—That creature thinks, at all events, it may escape; sport is not so cruel; nothing is so cruel as vivisection. The creature is helpless, there is no pitting of power against power, there are no chances of escape; there are none of the conditions that there are in sport.

6537. Then it comes back to this, that there is no standard which you can suggest that we can apply?—We can apply the general human standard of what is right and what is wrong, of what is kind and what is cruel.

6538. But then when we get a difference between human beings we lose at once any general standard?—No, I think people who have been brought up to vivisection cannot judge. If you start as a student by vivisecting, I think it blunts the humane sense. Consequently, men who have been accustomed all their lives to vivisection are not persons of ordinary humane sentiment. I do not wish to accuse anybody of wanton cruelty. I think it is habit.

6539. I wish to ask you some other questions about one or two points in your evidence. You referred to the question of hypnotism in your answer to Question 5339, and you said "I think very likely that, if you know anything of the phenomena of hypnotism, very often these animals must be entirely under the hypnotic influence of the man who operates"?—Yes, I have been told by an expert hypnotiser that it is

possible to hypnotise animals. Suppose a man is experimenting upon the brain of one of the lower animals, how can he tell how far his influence affects the results he gets upon that animal?

6540. Would the hypnotised animal feel pain?—Not if it were hypnotised with the intention of preventing pain.

6541. You think it would be unconscious hypnotism in the case of vivisection?—Unconscious hypnotism. Dr. Esdaile, I think it was, has written a book on the subject. He performed a number of hypnotic experiments by which he anaesthetised his patients by hypnotising them; but then his intention was to keep them free from pain—to prevent sensibility to pain. But the intention of these experimenters would have no relation to pain, but would be to tell whether they will get certain movements from stimulation of certain areas of the brain. I think that if an animal is hypnotised, in stimulating a certain area of the brain the operator may unconsciously suggest to the animal, to the subconsciousness of the animal, that certain muscles should move in response to the stimulation of a certain area.

6542. Then would the same thing apply to surgical operations performed by a surgeon on his patient, operating in the ordinary way with anaesthetics I mean?—In the case of a surgeon operating on his patient he is not dealing with the consciousness of the patient at all; the anaesthetist would be influencing the patient in that direction, if there were any influence in that direction. The surgeon is occupied with the limb or the organ that he is removing.

6543. I only wanted quite to understand what your view was. There is one other point. I want to know how far you carry your argument to. In answer to Question 5343 you said this: "Disease is a natural operation of Nature, and when we attempt to thwart the operations of Nature I think we are trying to put back evolution." Does not that argument carry you to this extent, that we ought simply to leave disease alone?—I do not think so at all.

6544. Are we not thwarting Nature?—Nature has given us not only intelligence for the treating of disease, but has given us drugs and other medicaments for treating disease. That answer of mine simply applied to artificial prevention of disease by attempting to immunise patients by means of sera and vaccinations.

6545. Do you object to surgical interference?—Certainly not; it is most valuable and necessary at times.

6546. Then your statement, as expressed, went rather further than you intended?—It was not clearly expressed.

6547. Then I want to ask you one question about this statement of yours: "When we put the blood of one of the lower creatures into the human being, we confer not only the immunities of that lower creature, but the susceptibilities of that lower creature." Does that refer only to susceptibilities to certain diseases?—I do not know whether I might not go further. Seeing that physiology and psychology are so absolutely allied that we cannot dissociate them, the question is whether we may not also confer the moral affinities of that lower creature.

6548. Its character?—I do not know. These things are so complex that they are dangerous to deal with.

6549. Can you refer us to any actual evidence on that?—Yes. Dr. Montgomerie Paton was experimenting on his patients with the plasma (the fluid portion) of the blood of horses and sheep and various other animals, and he found that when they were taking, even by the mouth, the plasma of these creatures it made them susceptible to the diseases to which these creatures are most susceptible.

6550. A disease like rinderpest, for instance?—No, I do not say that. But, for instance, if while they were taking the plasma of a creature that was subject to influenza the patients got influenza, they got it in a severe and dangerous form. He states that distinctly.

6551. (Sir John McFadyean.) May we have that quotation?—I think I have it here.

6552. It seems extraordinary?—He states that distinctly.

6553. (Sir William Church.) Who says that?—Dr. Montgomerie Paton. He performed a great number of experiments with antidipltheric serum, and with the plasma of horses and oxen and sheep, and he found that

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if his patients, while they were taking this plasma, took a disease to which the animal was subject, and was very susceptible, the disease ran an unusually severe and dangerous course, even when he was giving the plasma by the mouth.

6554. (*Sir John McFadyean.*) Is the disease specified—was it glanders?—No, it was a disease to which both human beings and animals are subject.

6555. To which the animal was specially susceptible?—To which it was specially susceptible.

6556. What was the disease; can you find out for us?—I mentioned catarrh and influenza to you just now. Then, again, may I read this to you?

6557. (*Sir Mackenzie Chalmers.*) If you please?—In the "Journal of Pathology and Bacteriology," Volume VI., No. 3, page 307, the following case is recorded: A horse which had been placed in the Brown Institute (this does not quite deal with it, but still, it is a very important thing) to be treated for a sprained leg had blood drawn from its jugular vein. Its blood, the operator records, "was full throughout of actively moving bacilli. This result made me suspect that the horse may have been at some time used for the raising of antityphoid serum, and afterwards sold for work." That is immunity. This creature, through long injection of typhoid bacilli, had become what we call immune to typhoid bacilli; that is to say, it allowed actively moving bacilli to be in its blood without attempting to throw them off by the natural processes of disease. Then in the "Journal of Medical Research" for August, 1905, Messrs. Duval and Lewis record results of blood cultures in five cases of general septicæmia. They say that in a case of infection by the bacillus mucosus capsulatus the blood was examined on admission, during the course of a five weeks' illness, and on dismissal. When dismissed the patient was well, and in two weeks was at work, yet his blood contained more organisms on leaving the hospital than were present on admission. That is artificial immunity. That is the sort of immunity that modern preventive medicine is trying to impose upon us.

6558. That was not quite what I was asking?—No, it was not, but I was very anxious to read it. I think I can find what you mean.

6559. (*Sir William Church.*) Will you pardon me for saying that is not to your point. You have not shown there (neither apparently does the extract you have read show) that these organisms which were in the blood of the horse had anything to do with the disease; because, for instance, in diphtheria a horse that is infected in the way we are does not have diphtheritic bacilli in its blood. We should like to know what organisms your informant found in the blood; perhaps you can get that information?—The experimenter was Dr. Samuel Shatlock. He says they were typhoid bacilli.

6560. (*Sir Mackenzie Chalmers.*) We have got rather far from what I was asking you?—Yes. In his book, "New Serum Therapy," Dr. Montgomerie Paton, who claims to have cured all things, from appendicitis to broken limbs, with antidiphtheric serum and with the blood plasma of horses and sheep and oxen, states that in conferring what he considered to be the antitoxic and resisting powers of the animal whose serum he was using, he conferred also the special susceptibilities and lack of resisting power against certain diseases which were characteristic of that animal. For example, horses and sheep are specially susceptible to catarrh and influenza, so he says, and he found that if, while taking, for the cure of some or another disease, the plasma of horses or sheep, his patients were attacked by catarrh or influenza, the course of such catarrh or influenza was unusually severe. Here, now, is evidence that whether or not the immunities of the lower creatures can be transferred, at all events their susceptibilities can.

6561. Who is Dr. Montgomerie Paton?—I do not know who he is, but I know that his book is considered one of the very latest things in serum therapy.

6562. (*Sir John McFadyean.*) Is he an American?—I think not. I believe he is practising in England. I can supply you with that information.

6563. (*Sir Mackenzie Chalmers.*) Perhaps you will kindly do so?—Yes, I will.*

6564. And you think that though he confines it to those two specific cases, it may go much further?—He gave other examples, but I think that in addition to such immediate results, we may also get grave later constitutional results. For example, if a person burns his hand badly, it produces a scar. To the end of his life nature forms his skin on the pattern of that scar. Well, suppose we inject the blood of some creature into a person and alter the natural character of that person's blood, how can we say that for the rest of his life his blood will not be formed on the pattern of that altered blood which resulted from the injection of the serum?

6565. I see your point. Do you confine your objection to lymph?—To the whole thing—the whole admixture of bloods. To inoculate a healthy human infant, the child of clean-blooded, healthy human parents, from one of a hundred sores artificially produced on the abdomen of a calf is a disgusting and dangerous practice.

6566. Do you think it has not stopped the spread of small-pox?—Even supposing it has, I do not think that justifies it. And I do not think there is proof that it has.

6567. You have seen bad cases of small-pox, of course?—Yes.

6568. Have you ever known a nurse or doctor who has been properly vaccinated catch it?—Again supposing that small-pox is a part, which we do not understand, of the great plan of Nature which we do not understand—

6569. Of removing superfluous beings?—No, not at all; but supposing it plays some important developmental part—

6570. That is hypothesis?—Yes; but in China you have this notable example. In China small-pox is very common, but in the parts of China where there is no small-pox there is leprosy. How do we know that small-pox was not a substitution on the part of Nature for leprosy? Leprosy was common in England in the sixteenth and seventeenth centuries.

6571. But still, at any rate, while we have diminished small-pox in England we have, happily, not increased leprosy?—We have not, but we cannot say that we have not dangerously interfered with the plan of Nature in some way that we do not understand. For example, we may have increased the tendency to cancer. On the face of it, it is an abnormal practice.

6572. But other animal secretions you do not object to putting into the human being?—Yes.

6573. For instance, milk?—Oh, well, that is a food. That is a benign and beautiful secretion.

6574. You draw a distinction between the lymph and milk?—You see, Nature causes the animal to secrete milk for the rearing of its young.

6575. Milk for one purpose and lymph for another?—I do not think that is a product of disease, is it? We cannot call it a morbid principle.

6576. Would you confine what you have told us then to morbid lymph?—To the products of disease.

6577. Would you object to the transfusion of blood in itself?—Certainly. The transfusion of blood is not used now, or very little, because it has been found that simple saline solutions are all that are needed.

6578. I understand. I do not want to go into details. Of course, as medicine progresses new remedies are found out and new methods of treatment. You do not think it is justifiable to try them on animals before they are tried on human beings? Take a new soporific, for instance?—I think it is valueless, because the experiment has always to be made on human beings afterwards, therefore why not begin on human beings—with infinitesimal doses? We can begin with the smallest doses. We know more or less by analogy whether a drug is poisonous.

6579. But most drugs operate in the same way on an animal as on a human being, do they not?—No, they do not.

6580. Take strychnin?—Strychnin does; but morphia acts as a convulsant and produces tetanic symptoms in a number of animals.

* Miss Kencaly subsequently wrote: "Dr. D. Montgomerie Paton is an L.R.C.S. and L.R.C.P. of Edinburgh. In the Preface to his book, dated 1906, he states that his experiments were done mainly in Australia. He thanks Dr. J. W. Bull, Bacteriological Laboratory, Melbourne, for assistance in his work. I have been unable to learn anything further about him."

6581. Cold-blooded animals?—No.

6582. But if you take prussic acid, you find the same results in animals as in man, do not you?—I do not think it is constant enough for us to be able to apply our results on animals to man without having first experimented on man. And there is no danger in experimenting on man in small quantities and noting the symptoms. Any young student who is worth his salt would be glad to make little experiments of that kind, and it would give him an interest in his work.

6583. (*Dr. Gaskell.*) Do you think there is any difference between small doses and large?—I do not object to his taking a large dose, but it would be safer for him to begin with a small dose and gradually to work up, and so find the physiological dose.

6584. You said that countless numbers of frogs were operated upon tied down to a board without anæsthetics?—I believe it is not usual to anæsthetise frogs.

6585. You know, of course, that that is against the law?—Is it against the law to operate on frogs without anæsthetisation? Do they anæsthetise frogs?

6586. Is it not against the law?—I understood that frogs were not anæsthetised.

6587. Frogs are exempt from this Act, are they?—Well, it is a great astonishment to me if it is the custom to give chloroform to frogs.

6588. I did not say that it was the custom to give chloroform to frogs. I only asked whether frogs are not amenable to the same law as dogs and cats?—But are they amenable to the same law if they are not anæsthetised, because dogs and cats are supposed to be anæsthetised?

6589. Most emphatically they are?—Well, then, we do not carry out the law when we do not anæsthetise frogs.

6590. Do you not know that frogs are always pithed?—I know that frogs are sometimes pithed.

6591. Do you not know that they are always pithed if they are not given anæsthetics?—As I was saying just now, I think that there is not sufficient evidence that the power of sensibility has been lifted in frogs to the brain, because frogs are capable of intelligence, and are evidently sensitive to pain after their brains have been removed.

6592. The spinal cord, you mean, is sensitive to pain?—I do not know about the spinal cord. I know that they perform acts of intelligence. A frog without its brain will swim and catch flies and perform other acts of intelligence.

6593. Do you understand what pithing is?—Yes, I understand what pithing is. It is spiking the medulla.

6594. Can frogs swim and catch flies after their medulla has been spiked?—They may be able to feel.

6595. That is not what I asked you. I asked you can they swim and catch flies when their medulla is spiked?—Perhaps not, but they may be able to feel.

6596. With the spinal cord? There is nothing left in but the spinal cord?—But can you be sure that the spinal cord of a frog is not capable of sensibility?

6597. That is the question I was going to ask you. You are willing to say that?—I am not at all content to believe that they are not.

6598. Have you ever seen cases of paraplegia in man?—Yes.

6599. Can you not get reflex action by tickling the soles of the feet?—Yes.

6600. Then they also are sensitive?—It does not follow.

6601. Is there any difference in the evidence?—We cannot argue from frogs to man. If you remove a man's brain he will not be able to catch his food, and a frog will be able to catch its food, and will be able to swim.

6602. Does it not depend upon what you mean by the word "brain"?—Yes, but the word "brain" does not mean the same thing in a frog that it means in a human being.

6603. Yes, the same part is there; the medulla oblongata is there; the mid-brain is there; the hind brain is there; and the fore brain is there, just the same as in man?—How, then, is it that a frog will perform acts of intelligence after its brain has been removed?

6604. But you say that the acts of intelligence are

that on putting an acid paper on the leg of the frog it removes it?—And it can catch its food.

6605. If it can, that is done by the spinal cord without any brain at all?—I do not think there is sufficient evidence to prove that it cannot also feel.

6606. I will take that as your answer. Do you believe in evolution?—Distinctly.

6607. Then when you say that all observations on cats and dogs and rabbits, and so on, are useless with regard to man, would that also apply to monkeys?—Yes.

6608. You do not think that monkeys are sufficiently close to man to be of any value at all in that respect?—I think that probably there are 3,000,000 years of difference between a monkey and a man.

6609. And you think that is enough evidence for your theory?—I think that is enough to justify it.

6610. Although every muscle is the same?—I do not think that is of any consequence. Muscles are not the chief thing we are dealing with.

6611. Although the convolutions are largely the same in the brain?—They are rudimentarily the same.

6612. They are very much the same. You do not think there is any point in that, then?—No.

6613. Now I want to come to your evidence. I understand that you have specially paid attention to the brain and ductless glands. Is that not so?—I dealt with those in my evidence.

6614. You have specially studied them, and paid attention to them?—Yes.

6615. And you think that in neither case has anything of value been shown by vivisection?—I think nothing of value has been shown.

6616. You think that no advance has been made in our knowledge of the brain or ductless glands?—I think nothing of value has been gained.

6617. Then I wish to go right through your evidence, if you do not mind?—Will you let me read this?

6618. I think it would be better for me just to ask you what I want. For instance, you say that no knowledge has been acquired with respect to the spleen, and you quote from Professor Starling?—Yes.

6619. But Professor Starling in his "Elements of Human Physiology," on page 502, says: "We must indeed look upon the spleen as the great blood-filter, purifying the blood in its passage by taking up particles of foreign matter and effete red corpuscles. The process of phagocytosis, which was described under inflammation, is in the spleen a normal occurrence." Is that absolutely no knowledge?—That is knowledge, but it is knowledge that was not gained by experimental physiology.

6620. That, at all events, is not what I understood you to say. You said that no knowledge had been gained?—I believe in the knowledge which we gain from pathological observation and clinical observation, and subjective observation.

6621. Then I will go on to your next piece of evidence. "In Kirkes's Physiology are mentioned rhythmical contractions and dilatations of the spleen as obtained by the oncometer (an appliance for enclosing the organ of a living creature). But as Starling does not even allude to these, I take it that they have since been discredited?"—Yes, I think you will find that that is so. I looked carefully to find it in Starling. I may have made an error. I am not infallible.

6622. This is what Starling says: "If the spleen be enclosed in a plethysmograph, or splenic oncometer, and its volume be recorded by connecting this with the oncometer, it will be seen that it is subject to a series of large, slow variations, each contraction and expansion lasting about a minute, and recurring with great regularity," and then a figure is given showing these undulations of the spleen?—May I ask what edition is that? Mine was the 4th edition.

6623. This is the 4th edition?—1900?

6624. 1900—the same year?—I am very sorry that I missed it.

6625. I presume that you did not quite understand what he was talking about?—I understood it perfectly. But is that under the head of the spleen?

6626. That is under the head of the spleen?—I am very sorry to have missed it.

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6627. I think you cannot possibly have understood the meaning of these tracings?—Yes, I understand the meaning of tracings.
6628. He gives more than Halliburton (Kirkes), because he gives a tracing as well?—I cannot understand my having missed it.
6629. But you go out of your way to be sarcastic; you say that you take it that the rhythmical contractions and dilatations have since been discredited because Starling does not even allude to them (*handing a book to the witness*). He gives a more elaborate description than Halliburton does?—I am not quite sure about the date of it then.
6630. That is 1900. You gave us 1900?—Did I give 1900 about Starling?
6631. Yes?—I am very sorry. (*After referring to the book.*) I cannot believe this is in my copy. I am very sorry—it was purely a mistake. I have not made many such mistakes, I am sure. I hope you believe that I would not for anything have made such a statement otherwise.
6632. I thought that because the word rhythmical was not there you did not understand it?—No. I am almost sure it is not in my book.
6633. Then we came to the question of these ductless glands. Sir William Church asked you about the evidence of centuries, and I understood you to say that although they had been examined and explored by physiologists by means of countless vivisections, yet we know nothing about, I will take to begin with, the suprarenals?—Yes.
6634. Have you any notion when the suprarenals were first investigated?—I have not the slightest doubt that they have been investigated ever since vivisection began; I think we may take that for certain.
6635. That they have been investigated and experimented upon?—I have not the slightest doubt that they have always been experimenting on them. We have evidence of experiments on the pancreas in 1660, so we may be quite sure that they were experimenting on every gland in the body by means of vivisection. It is most unlikely that they left one uninvestigated and investigated others. It is simply because they have obtained no results that they do not mention them.
6636. That is your own private opinion?—I think we may take that as absolutely true.
6637. Are you aware that the first, or very nearly the first at all events, of these investigations of the suprarenals were purely histological?—I suppose purely. Was there anything before Addison?
6638. Leydig in 1851 examined these glands histologically?—A very rational method of examination.
6639. And in 1851 he found out the medulla and the cortex of the glands?—Yes.
6640. And later, or in 1865, Henle found out that certain cells in the medulla gave a reaction with salts of chromium; did you know that?—Those were not experiments on living animals—
6641. Wait a minute, I am coming to that?—You asked me, do I know about a chromic reaction. Yes, I do, certainly.
6642. Is there any evidence whatever that the knowledge of the functions of the suprarenals was known in the slightest degree till long after Leydig and Henle's experiments?—I think that the first knowledge of any value that we have was from Addison's clinical observations as to the serious symptoms that resulted in human patients from disease of the suprarenals.
6643. But you are not aware that until Schäfer's experimental work, we did not know anything about the substance called adrenalin?—I do not consider that at all a valuable discovery. I do not consider it a valuable discovery because it does not tell us about the functions of the suprarenals—that is what we wish to know.
6644. Does it not?—I think not. Remember, as you know, of course, we cannot even say that proteids occur as proteids in the living human body. We know so little as that. We cannot distinguish in the laboratory between proteose, let us say, the white of an egg in a half-digested state, and the most venomous snake poison. What do we know?
6645. Since Schäfer's work in 1894, has there not been a large amount of work done on these glands?—I do not know that it is very productive work.
6646. I did not ask you that. I asked you, has there not been a large amount of work done on these glands?—I believe so.
6647. Have you ever heard of Kohn's work?—Yes.
6648. What do you think did Kohn mean by the paraganglia?—Surely that was histological observation.
6649. That is not what I asked you. I asked you what was meant by it?—I do not know what he means by it.
6650. You have not read his work very much?—No, I have not read his work very much. I simply know that he has performed operations.
6651. What is the difference between the medulla and the cortex of the suprarenals?—If you mean the difference of function, nobody knows.
6652. No, I mean the difference?—It is a histological difference. I could have told you when I was a student, but I cannot say that I have kept up to all the latest physiological details—largely because I do not consider them of very much consequence, as they are chiefly obtained from dogs and rabbits.
6653. But you are professing to give us evidence here to the effect that investigations upon the suprarenals have led to no result?—Yes, I was quoting in my last evidence.
6654. And you have, you say, studied the question of the glands and the brain; I am only taking them because you have specially studied them?—Yes.
6655. Are you aware, then, that the medulla and the cortex in the suprarenals arise from two different glands?—I am aware that we know nothing about the separate functions of each; and the function is the main thing.
6656. Are you aware that the functions of the medulla are different from the functions of the cortex?—I have not the slightest doubt about it, because the histological structure would tell us that there is a difference. But what that difference is we at present are quite unable to say.
6657. What is the nature of this chromic acid reaction—do you know that?—I do not know it. As I told you, I have not kept up my student's knowledge of physiology.
6658. You are not aware, then, that in consequence of Schäfer's experimental work, or even just about that time, a large amount of work has been done showing that this chromic reaction exists always in certain cells?—I do not consider it of the slightest value if it was obtained under mutilation or anæsthetization.
6659. No, it was obtained simply histologically?—I value histology.
6660. I did not ask you whether you valued it. I asked whether you knew anything about it?—I know that all histological work is most valuable—function is so allied to structure that histology, or microscopic anatomy, is most instructive.
6661. May I ask you, are you aware that the suprarenal is composed of two parts, one of which comes from a series of segmental organs that you can trace right down to the elasmobranchs, the medullary part, and the other is the interrenals, which are totally different glands in the fishes? You do not know about that?—I think that is very valuable, because it is the result of histology.
6662. Then you go on to say: "You see that we know nothing about the carotid and coccygeal glands"?—I quoted Starling for that.
6663. But you say that you have studied this?—I can only study the authorities upon the question. I have made no experiments myself.
6664. But the date is 1900?—Yes.
6665. Do you think that nothing has been done since then?—I do not know.
6666. Do not you know that in consequence of Schäfer's experiments an enormous amount of work has been done, and the whole question has been cleared up in a marvellous manner? You do not know that, because you only take what is written down in the elementary text books, and you come here and profess to give us the opinion that nothing has been done and no advantage has been derived?—I take the "Journal of Physiology" for August, 1906, and I find from that that we know nothing about the suprarenal glands.

6667. You are referring to Elliott and Tuckett's paper?—That is the paper I read.

6668. I will come to that in a minute. You do not know, then, that the carotid and coccygeal glands have the same substance in ~~them~~ as the medulla of the suprarenals?—That is histological.

6669. And that that substance forms adrenalin; you do not know that?—No, I did not.

6670. You know that adrenalin has been largely used in medicine of late years, and has been very valuable?—I do not know that it is valuable. I know that we have so many other agents that produce the same effect.

6671. But we cannot say now that we know nothing more about the carotid and coccygeal glands, because they are part of the same great series of paraganglia?—I quoted Starling and Kirkes, to tell you—

6672. I only want to know whether you had taken the trouble to find out anything about the literature of the present day?—I have taken the trouble.

6673. Apparently you are ignorant of it. Then I come to the question of the brain. You there objected with respect to the question of blindness, caused by the destruction of the cortex, that there is a difference of opinion, that one set of observers suggested the occipital lobes, and the other the angular gyrus?—Yes.

6674. Can you tell me whereabouts the angular gyrus is in connection with the occipital lobes?—Is it fair to put me through an examination in anatomy?

6675. It is fair, because you come here and distinctly tell us that no knowledge worth anything has been obtained by these methods?—I quote Ferrier for that.

6676. I take these two cases of brain and glands, because those are the two cases you profess to have studied?—Let me read you this from Ferrier himself.

6677. I only want to know if you can tell me where the angular gyrus is in connection with the occipital lobes?—They are close to one another.

6678. That is all I want; they are close. I want the Commission to understand that, because they might have thought they were in quite different places. So that really it does not come to much more than that the cortical centre of sight has, in your opinion, not been defined very closely?—I think it has not been defined.

6679. And yet they are close; they are touching each other?—But it is also stated that nerve action is by contiguity, not by continuity.

6680. Are you aware that the fibres concerned with vision from the occipital lobes pass under the angular gyrus?—Yes.

6681. And that, therefore, if the angular gyrus is destroyed a little too deeply, that would account for the loss of vision?—I think that is the result of destroying parts instead of judging by clinical results. That is just the question. In the crude method of destroying the brain you may be destroying most valuable evidence.

6682. Do you not understand that the discrepancy is simply this: that some observers say that the angular gyrus has nothing to do with vision, and that is because they only destroy the surface of it, while others say that the angular gyrus has everything to do with it, because they went a bit deeper, and so destroyed the fibres from the occipital lobes?—I think that is precisely where the danger of these extirpations comes in. Things which are not related physiologically may be anatomically near to one another.

6683. But you said that we had no certainty. I say that we have?—I take that from Ferrier.

6684. What date was that—that was a long while ago?—It was a good while ago, but if he could get no results ten years ago why should we depend upon results that are got to-day? Ferrier says: "Nor do the facts of experimental physiology seem so consistent with themselves or with the undoubted facts of clinical research as to inspire us with unhesitating confidence as to their accuracy or as to their applicability to human pathology."

6685. That is a general statement?—It is a very valuable statement.

6686. I want to stick to the point. In your criticism of these brain experiments I notice that you confine yourself to the localisation of special senses?—I was not allowed to read all my paper on the subject.

6687. You confined yourself to the sense of vision, the sense of hearing, the tactile sense, and the sense of smell and taste; that is all we have in evidence?—Yes; I was not allowed to read all about the brain.

6688. I want to know whether the same discrepancy and the same difficulty apply, in your opinion, to the centres of the motor activities?—I think so distinctly, because Professor Ferrier says that these are centres of motion, and Professor Munk says they are centres of the sense of motion, which is quite a different thing. Munk places the true motor-centres in the spinal cord.

6689. That is not the point. Do you consider that the same discrepancy occurs nowadays with regard to the localisation of movements in the cortex for different parts of the body, the hands, feet, legs, and so on? Why did you not mention these as well as the others?—I was not allowed to go right through with my reading. I was stopped when I was going on to those.

6690. Do you not know that there is absolutely no doubt now with respect to the different parts of the cortex concerning the movements of different parts of the body?—I think that wherever it has been proved it has been proved by clinical observation.

6691. But as the result of experiments on localisation of the cortex in the Rolandic area?—These have not been proved.

6691A. Not proved?—They are still disputed.

6692. Who disputes them now?—Claud Bernard, who was a most brilliant observer, did not believe in them.

6693. The cortical localisation was not even thought of in Claud Bernard's time?—Goltz is another.

6694. That is what I say. At the time of Goltz's experiment there was that same sort of doubt which you put forward with respect to some of these sense centres; but now the doubt is cleared up absolutely?—Then why are there differences in the brain-maps of different observers?

6695. Do you know that tumours have been diagnosed and cut out by surgeons in consequence of this knowledge with perfect accuracy?—There is a brain expert coming who will tell you, I think, that that is not so, that the brain in various persons differs with regard to the bones of the skull and the position of the sutures.

6696. We shall have Professor Horsley here, and he can tell us, too, from his point of view. You do not know also, with regard to the cerebellum, that the localisation has been so completely made out by experiments there that Sir Victor Horsley can remove tumours in the cerebellum by knowledge of the symptoms?—I think that the function of the cerebellum has been shown by disease.

6697. I should like just to go on shortly?—This is what Ferrier says: "One great fallacy has been the assumption that the results of experiments on frogs, pigeons, and other animals low in the scale are at once capable of application to man without qualification."

6698. I do not think that has any bearing on what we are speaking of. Suppose we pass on to the next matter. You referred to some observations in the "Journal of Physiology" for August 10th?—Yes.

6699. The first that you dealt with was Mr. Bainbridge's?—Yes, about lymph.

6700. You say that Mr. Bainbridge got no results?—Shall I read you what his results were?

6701. No; I know them, thank you. You give there a quotation from Mr. Bainbridge: "The post-mortem lymph flow after the intravenous injection of peptone is due either to filtration through abnormally permeable capillaries, or to increased metabolism in the liver, the former being the more probable." His experiments were mainly on the injection of dextrose, not peptone; did you know that?—Yes, dextrose—he states that himself.

6702. I am aware of that. That is simply one experiment on peptone in which he got no definite result. But it is not true to say that he got no definite results on his dextrose experiments, and the object of the paper

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was the dextrose experiments?—Can you tell me what results he got from dextrose?

6703. You can read it in the book, and you will see. Do you consider that it is of no importance to know whether the post-mortem flow of lymph is simply a mechanical action, or is the action of living cells—that was the object of his experiment?—The power of sustaining osmotic tension is the characteristic of the living cell. With death this power of sustaining osmotic tension is lost. All the fluids of the body proceed to come to an equilibrium just as do fluids in inanimate structures. In this coming to an equilibrium various phenomena occur according to the permeability of the various structures, according to the pressure within them, and perhaps according to the secretory power left in the cells.

6704. Yes?—But he did not prove to which of these conditions his results were due.

6705. He proved that it was a question of venous pressure?—The fluids, in coming to an equilibrium, may ooze as tears from the eyes, as saliva from the mouth, as lymph from the thoracic duct. The phenomenon has an interest, but it can be observed upon any post-mortem table without injecting dextrose and extract of strawberries into the veins of living dogs.

6706. That shows that they live after death for some time?—Yes.

6707. And valuable results have occurred from that knowledge?—Yes.

6708. That is to say, people have recovered when they were thought to be dead?—Yes.

6709. Therefore it is an advisable thing to find out whether or no in each case this activity is due to the tissues, or whether it is due to some mechanical action?—Yes.

6710. That is therefore in my opinion, and I should have thought in yours, an experiment that might be worth doing?—Not by the artificial injection of something which is not normally in the blood.

6711. Let us pass on to another one—that is, the experiments in America on the injection of various tissues?—The injection of shark and squeteague?

6712. Yes. Do you mean to say that there is no valuable knowledge to be obtained by the injection of extracts of tissues?—I say that on the face of it there is no valuable knowledge to be obtained by injecting an extract of shark into the blood of a living dog.

6713. I said extract of tissues?—Extracts of any tissue of a shark.

6714. Are you aware that we have knowledge that by means of the injection of extract of the thyroid gland valuable relief has been given in cases of cretinism?—And very dangerous symptoms, too, have been produced by injecting extracts of the thyroid.

6715. That is not the point. Are there not plenty of cases where extracts—adrenalin is another case—have been shown to be of value? Your objection, I understand, is that it is no use injecting extracts of the tissues of cold-blooded animals?—If we do that sort of thing our science will never be complete until we have injected extracts of every animal under the sun, and combinations of extracts of every animal under the sun.

6716. You are not aware, apparently, that in the suprarenals of the elasmobranch fishes you can get out precisely the same substance as adrenalin, and that the injection of suprarenal extract from those cold-blooded elasmobranchs will produce the same results in any animal you like of a vertebrate nature. Do you not understand that there is a continuity between all members of the vertebrate kingdom that we express by the term "Evolution," and that therefore it is worth while seeing whether that continuity is confined to form only, or whether it does not also apply to the chemical nature of their tissues?—I think that nature has given us so many beautiful healing medicaments, which we do not understand and which we ought to understand better, that it is quite unnecessary and quite absurd to go for our medicines to sharks and starfish.

6717. Then you say further that in those same experiments Dr. Percy Dawson has shown the valuelessness of them, because the experimenters did not measure the systolic and diastolic pressures?—No, Dr. Percy Dawson says—

6718. You infer that from what he says?—He says, there is, perhaps, no one factor which has done more to throw into confusion the literature on the subject of blood pressures than the use of the term "blood pressure" without specification as to the sort of blood pressure under consideration.

6719. And you say, therefore, in your answer to Question 5316: "According, therefore, to Dr. Percy Dawson these experiments of Dr. Brown and Don Joseph, because they do not distinguish between systolic and diastolic blood pressure, are absolutely worthless, even from the laboratory point of view"?—You will find all the way through that they speak of blood pressure; they do not speak of diastolic or of systolic blood pressure.

6720. Do you know what diastolic and systolic blood pressure is?—I think I do.

6721. In what way does Dr. Percy Dawson use the term?—He simply states that the systolic blood pressure may exceed the diastolic by so much as a hundred per cent.

6722. But what does he mean?—That these two pressures may vary quite independently of each other; and these observers do not distinguish between them.

6723. In what animal?—In man; he is speaking of man.

6724. He is speaking of man?—Yes.

6725. Do you mean to infer or to tell me that when a manometer is placed in connection with the blood vessel of any animal, you cannot measure accurately the systolic and diastolic pressure—the whole blood pressure—in every respect?—But they do not say a word about it; they do not mention the differences between the diastolic and systolic pressures, and if there had been any value in the paper—

6726. Can you mention the differences between diastolic and systolic in blood pressure taken by the mercury manometer?—That is a detail.

6727. No, it is not. Do you not see that Dr. Percy Dawson is simply speaking of man, and that his remarks have absolutely no bearing upon experiments on animals in which you can put the mercury manometer in connection with the vessels. You cannot put the mercury manometer in connection with the vessels of man; you have to measure your pressures indirectly in other ways. But in animals it is the direct method of measuring that we use, and the systolic and diastolic pressures have absolutely nothing to do with the question considered by Messrs Brown and Don Joseph. Let us go to Messrs. Elliott and Tuckett's paper. I understand in that case that you think that the whole question of injecting pieces of guinea-pig was a useless question?—I take their own results.

6728. I want to know why you think they chose the guinea-pig?—Because the suprarenal gland is large in the guinea-pig.

6729. Which part of it?—The article tells us. I simply have to look it up to tell you.

6730. I can tell you; it is the cortex?—I believe it is.

6731. Because the cortex in the gland is exceedingly large in the guinea-pig?—Yes.

6732. You are aware that extracts of the suprarenal gland are used in medicine?—Yes.

6733. And you are also aware possibly that we know nothing of the functions of the cortex; perhaps you did not know that?—Yes, I do.

6734. That we only know the functions of the medulla?—How much do we know of the functions of the medulla?

6735. We know that it possesses the substance, adrenalin?—How do we know that it does? I think we do not know that it does. If we do not know that the protoplasmic cell exists as proteid in the living creature, we cannot say that adrenalin exists in the living creature as adrenalin.

6736. I said simply that we can get adrenalin out of the medulla. You were not aware of that?—Oh, yes I am. Please do not say I am not.

6737. And you cannot get it out of the cortex?—No.

6738. It is very necessary therefore to know whether the cortex has any function, or whether it is

harmless, is it not?—It would probably be valuable knowledge, but I think it is not to be obtained by vivisection.

6739. But the whole point of these experiments was to endeavour to find out what was the function of the cortex?—Yes, precisely.

6740. And they found out that in that case the cortex of the guinea-pig had a toxic action?—No, what they say is: "To speculate on its meaning would at present be idle"—the meaning of the great development of the cortex.

6741. That is not the question I asked you. They have found out that in the guinea-pig there is a large increase of cortex and that in those cases you get certain toxic results under the skin. You call those results septic?—They say so themselves.

6742. I do not think so?—I will read their statement from their article. "The experiments are inadequate to distinguish whether these differences should be ascribed to a specific cytotoxin or to different conditions of temperature and circulation. A patent fact is that the hæmorrhage and 'exhaustion' of the suprarenal glands are not due simply to the injection of another suprarenal into the animal's body; for were such the case it would be more severe with the rapid absorption from the peritoneal cavity. It must be a secondary result determined by the necrosis of the superficial tissues and by the resultant disturbance whether mechanical or chemical in the animal's economy."

6743. What has that to do with ordinary sepsis?—It means that the results that he got were the results of ordinary sepsis.

6744. No?—Certainly.

6745. Certainly not. Ordinary sepsis is bacillary infection; this was antiseptic treatment. It means that there is a toxic substance in the cortex and medulla of the glands of the guinea-pig?—They do not say so. They say the experiments are inadequate to decide this.

6746. But that is what they suggest, and that therefore, in preparing specimens for administration to man, it is advisable to know how far you can find out the functions of the cortex, so as to avoid any toxic thing. It is a most important investigation for the purpose of finding out the nature of the cortex?—I admit that the aim was important. What I contended was that the results are absolutely nil.

6747. What—to show that it was toxic on the guinea-pig?—They did not show that it was toxic on the guinea-pig. They simply showed that when glands were grafted into the subcutaneous tissues they decomposed and set up sepsis.

6748. They did not say that they decomposed?—How could you prevent them from decomposing?

6749. What should make them decompose?—You mean because there was no micro-organism?

6750. Yes, I do?—Then why do they speak of necrosis of the superficial tissues?

6751. Necrosis means simply some poison that is enough to kill them. That is what they mean by that?—I do not think they do—at least, with all due deference to you.

6752. I happen to know that it is so?—"These somewhat tangible restrictions" is how the writers themselves describe them.

6753. I think we had better not talk about it any more?—I hope you will realise that I speak with all due respect, if I am a little excited.

6754. I want just to ask you one or two questions in connection with Mrs. Cook's evidence, because Mrs. Cook told me that she was ignorant of these matters, and that you were the person to whom all her evidence had been given beforehand?—Oh, no. I did not read it before she gave it—at least, I read it an evening or two before, but I had nothing to do with its preparation.

6755. She told me that your committee had read it?—But we read it a day or two before. It was not submitted to us with the idea of correcting it. We all prepare our evidence independently, and I had nothing whatsoever to do with her evidence.

6756. She told us that she could not answer any expert questions, but I understood her to say that you would come and do that for her?—Oh, no; I said to Mrs. Cook before she gave her evidence: "I should very much like to read your evidence," and she sent it to me a day or two days before, but with no idea of my making any alterations in it.

6757. (Sir William Church.) She certainly led me to think that you had furnished her with the information, especially with reference to experiments, which she placed in her *précis*?—No, not at all. The only thing she quoted me for was with regard to the Open Letter.

6758. (Dr. Gaskell.) Might I ask whether you agree with some of her statements? She said, for example, that in one case a ligature round the optic nerve was tied as tightly as possible and the animal was kept alive; and she said, "This must have been a peculiarly agonising experiment"?—But please do not ask me about it; I should prefer not. You must not ask me to express an opinion upon the witness's statement.

6759. I only want to know whether you think that the optic nerve carries pain fibres in it?—In point of fact I believe not.

6760. That is all I want. I should also like to know whether in your opinion the surface of the brain is sensitive; does it cause pain when it is slightly pressed or when a piece is cut out?—It is generally supposed not to be sensitive; but Mrs. Cook was not dealing with the surface of the brain so much as with the operation to get at the surface of the brain.

6761. She is speaking with regard to Mr. Dean's experiments. She states that the brain was observed through a disc, which was removed at varying intervals and pieces of brain were taken out from time to time. This was done under anæsthesia, and then she states that the animals were kept alive for so many days?—I think it was the preliminary experiment that she was alluding to.

6762. No. The preliminary experiment was performed under anæsthetics. She speaks again and again in one or two places of an "agonising excitation." This agonising excitation was the stimulation of the cut end of the sciatic nerve. In your opinion would you say that that is an agonising excitation?—I think it is very difficult to say. You may say that it is only the central end that would convey pain.

6763. I certainly should say that?—There may be synapses and arborisations with other nerves which might conduct pain.

6764. I know of no evidence of any such communication. Do you know of any?—We have no evidence that there is not, have we?

6765. Absolutely. I was intending to ask you some other questions about Mrs. Cook's evidence, but after what you have said I will not do so. But as belonging to this Parliamentary Association, could you possibly tell me what evidence we are to have with respect to this statement of hers? She stated that certificates had been issued for allowing operative experiments without anæsthetics?—No. I must ask you not to question me upon her evidence.

6766. When she was asked if she could give instances she replied, "I feel sure I could supply you with them if you give me time to look them up." She was told "I wish you kindly would," and she replied, "I will send the answer at another meeting." We have been waiting for that answer ever since. You are not authorised to bring it?—No. Mrs. Cook is coming before you again I believe, and will be able to answer these questions then.

6767. Is she coming?—I believe so.

6768. I did not understand that. We should like to have that evidence.*

6769. (Dr. Wilson.) I have only two or three questions to ask you. I understand that Mrs. Cook prepared her own *précis* without your assistance at all?—Yes, and so far as I know no other member of the committee had anything to do with it.

6770. And I think when she referred to you in her evidence here as an expert, she merely meant that you were coming before us as a medical witness?—

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* Mrs. Cook had, in fact, written to the Commission stating that she was unable to furnish the details alluded to, without access to the Home Office files; and requesting to be heard again.

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No, it was in connection with the Open Letter that she mentioned my name.

6771. Then I may, I think, safely gather from all the evidence which you have submitted to the Commission that, apart from the question of utility, you object on ethical and moral grounds to animal experimentation of any kind?—Absolutely.

6772. And that of course would include vaccination as a protection against small-pox?—Certainly, I think that the admixture of bloods is a most dangerous practice.

6773. Whether that vaccination is by calf lymph or by human lymph as in arm-to-arm to vaccination you would still object to it?—I would still object.

6774. And from your wide reading you also say that you are impressed with the futility of all these experiments?—Yes. I think that at least three millions of years of evolution have taken place between the development of the rabbit, the dog, and

the guinea-pig and the development of man, and consequently that any results which we may get from the lower animals are not results which are applicable to human kind, and therefore are of no value as regards human kind.

6775. So that I may say that the moral aspect of the question which you take up is supported by your wide reading and the judgment which you are enabled to pass upon it as regards results?—I think so.

6776. And what do you think would be the effect upon medicine if all experimentation upon animals were prohibited?—I think that it would give the finest possible impulse to medical science; that we are surrounded by all these problems of disease and degeneration and suffering in human kind, and that if we were to devote our attention to man, and to all the valuable human material surrounding us instead of wasting valuable time and talent upon dogs and guinea-pigs, we should make rapid and immense advance in the relief of human suffering.
